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OF THE

ROYAL SOCIETY

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FOR THE YEAR MDCCCXXIV.

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MDCCCXXIV.

ADVERTISEMENT.

The Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable that a Committee of their members should be appointed, to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions;* which was accordingly done upon the 26th of March, 1752. And the grounds of their choice are, and will continue to

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be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the Chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they received them, are to be considered in no other light than as a matter of civility, in return for the respect shown to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

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The Meteorological Journal will appear in the Second Part of the Philosophical Transactions.

THE President and Council of the Royal Society adjudged the Medal on Sir Godfrey Copley's Donation, to John Pond, Esq. Astronomer Royal, for his various Communications to the Royal Society.

PHILOSOPHICAL

TRANSACTIONS.

I. *The Croonian Lecture. On the internal structure of the Human Brain, when examined in the microscope, as compared with that of Fishes, Insects and Worms. By Sir* EVERARD HOME, *Bart. V.P.R.S.*

Read November 20, 1823.

AT the time this Lecture was instituted for the discovery of the principle on which muscular motion depends, the principle was supposed to be inherent in the muscular fibre itself: the numerous dissertations, therefore, which are registered in the Philosophical Transactions, are in general so many investigations into the properties of muscular fibres.

This part of the subject may be considered as completely exhausted, although the principle on which the motion depends is not yet made out. This leads me to believe that we must extend our enquiries to the structure of the brain and nerves before we can arrive at it.

It is now, I trust, universally allowed by physiologists, that muscular motion cannot take place in living animals without the aid of the medullary structure of which the brain and nerves are composed.

Every enquiry into the more minute parts of the brain and nerves, as well as their ganglions, must be carried on upon the field of the microscope, and Mr. BAUER, in consequence of the many valuable discoveries which have been made by his skilful use of that instrument, has too much zeal for science to withdraw himself from such pursuits.

Considerable advance has been already made in this enquiry by Mr. BAUER'S observations on the component parts of the blood, the formation of the embryo of the chick, and the component parts of the brain.

These observations are registered in the Philosophical Transactions, and the communications have been of so recent a date, as not to require that I should do more in this place than refer to them.

In the present Lecture, it is intended to pursue still farther the anatomy of the human brain, and to compare it with that of fishes, insects and worms; in the hope that by the establishment of such a series of facts, we may throw light on the connection between the action of the nerves and the motion of the muscles.

The following circumstances explain the difficulties that are met with in the examination of the structure of the human brain.

The transparent elastic matter readily dissolves in water, and when in solution the globules become one confused mass.

The cortical substance containing serum, when it coagulates, is different in density from the medullary structure, and readily separates from it; the serum also in drying cracks into regular figures, giving it an appearance of a net work which is artificial.

The globules and elastic substance being in different proportions in different parts of the brain, have an influence upon the appearance put on by the coagulum, which is a source of deception, and makes the coagulum tear more readily in some directions than others, as if fibres in those directions existed, which is not the case.

The parts not being of one uniform density, makes it impossible for the sharpest knife to cut evenly; some of the globules must therefore be displaced, while others are not.

When the brain is immersed in alcohol and rendered solid, all these deviations from the natural appearance must be encreased in a greater degree, since the elastic substance is rendered opaque by coagulation, and all appearance of globules lost.

To obviate these sources of error, I requested Mr. BAUER to examine a small portion of the human brain in a recent state, composed of cortical and medullary structure, which had been immersed in distilled water: the surface of the elastic substance it is true, was dissolved, but what remained completely preserved its transparency; the appearance the surface put on when magnified twenty-five times, is seen in the annexed drawing, rows of globules from the circumference of the cortical substance are passing unbroken in straight lines into the substance of the medullary portion, which could not have been detected in any other mode of investigation.

I shall not prosecute farther this part of my subject on the present occasion, but proceed to the consideration of the brain of fishes, (that of quadrupeds and birds being in its structure so similar to the human brain, as not to require any notice of its peculiarities to be taken).

In the representation of the brain of the Tench, which is annexed, there is evidently a smaller quantity both of medullary and cortical substance in proportion to the size of the animal than in the bird, and its form is less compact, being made up of spherical nodules, medullary on the surface, and internally cortical: the basis of the brain is also nodulated, and in the centre is an oval cavity. The nodules are upon so small a scale that their internal cavities are not to be distinguished, but in the squalus maximus they are very conspicuous. I am now to point out the peculiarities of the brain in insects and worms; but cannot tread upon the same ground on which Swammerdam has preceded me, without paying a tribute of praise to that great man, who, labouring under such disadvantages as he must have done, in the age in which he lived, has performed so much, and in many instances has left nothing for those who follow him, but to bear testimony to the correctness of his representations and judgement.

There are some points in which he gave way to public opinion, and did not disbelieve what every one said must be true. I allude to his attempt to represent the eye of the garden snail at the point of the horn, which does not exist. He found black retemucosum, which he mistook for nigrum pigmentum, and a pellucid part which he took for cornea. To show this fallacy, I have annexed Mr. Bauer's representation of these parts. Swammerdam has given a faithful representation of the nerve, which might have undeceived him, it having no resemblance to other optic nerves, but being like those commonly met with going to tentacula.

It is curious, that long as has been the intervening period of time between Swammerdam and Bauer, no one has arrived at a like excellence in the use of the microscope; for certainly Poli (however splendid his plates) cannot be put in competition with either of them.

When Swammerdam died, and no one found himself equal to succeed him, a report was raised that his microscope was of a peculiar kind, and the mode of using it was lost at his death; so it is now with Bauer. Many applications are made to the mathematical instrument makers for a Bauer's microscope, by those who are not willing to believe it is their inability, and not the fault in the microscope, that prevents their arriving at his excellence.

In all the insect tribe I have examined, the brain is formed upon the same general principle, but very different from that of fishes; the brain is in one mass; it is too small to admit of a particular description, but contains globules; and from the readiness with which it dissolves upon exposure, there is no doubt of there being a fluid contained in it. Besides this, which is admitted to be the brain of the insect, there is another substance connected to it by means of two chords. This second part has been, I believe, usually called the first ganglion, but, when accurately examined, it is similar in its texture to the brain: the two chords which unite them are not properly nerves, since they are upon their first exposure turgid, but soon collapse. These two substances with their uniting chords form a circle, and surround the œsophagus; from the upper mass go off the optic nerves, those to the tentacula, tongue, &c.

From the lower mass go off the nerves to the upper extremities.

I shall therefore consider the upper as the brain, the lower as the medulla spinalis.

Below this is a regular line of ganglions, properly so called, being made up of a congeries of nerves, as the ganglions in the human body are now admitted to be.

The brain appears to be made up of two lobes. The mass I call medulla spinalis, is also made up of two portions, united together by the two lateral chords.

The ganglions down the body of the animal are united together by a double nerve.

The annexed drawings show this structure better than can be explained by verbal description. Among the insect tribe the brain of the Humble Bee stands first, as being largest in proportion to the size of the body of the insect. Swammerdam has given a representation of the Bee; it is in general correct, but not so in respect to the optic nerves.

The Moth and Caterpillar have the same kind of brain, medulla spinalis, and series of ganglions, as in the Bee: the parts in the Caterpillar are nearly of the same size as in the Moth, but in both they are smaller than in the Bee. Swammerdam has given a correct representation of these parts in the Caterpillar of the Silk-worm, but none of the Moth.

The Lobster is similar in the structure of all these parts to the Bee, and although they are smaller in proportion to the size of the animal, they are still so large as to be readily seen, and explain what is not so distinct in the smaller insects.

The Earth-worm has a brain and nerves formed upon a smaller scale, but made up of the same parts.

In the Garden Snail, the brain and medulla spinalis are upon the whole larger in proportion to the size of the animal than in the Bee; but in this animal, there are no ganglions, which may account for those parts being so large.

The clear and distinct representations which Mr. Bauer has given of these very minute objects, has made it unnecessary for me to take up the time of the Society in giving detailed descriptions of the different parts; indeed any thing that is to be said, had better be stated in the explanation of the drawings, than connected with the general remarks which form the Lecture itself.

The Snail having a brain of the same kind as the Bee, and the medulla spinalis having a similar structure, while the series of ganglions is wholly wanting, forms one of the most curious parts of this investigation. Having ascertained that, in all the animals, the structure of whose nervous system has been explained in the present Lecture, the brain is a distinct organ, varying in size it is true, till at last it is scarcely distinctly visible to the naked eye, but when examined in the microscope, found to consist of globules and elastic transparent matter, and more or less of a fluid, similar to the brain of animals of the higher orders; that there is also at some distance from the brain, a second substance of similar structure, connected with the brain by two lateral chords; and that this second part gives off the nerves that go to the different muscular structures of the body; I consider myself borne out in the opinion that this part answers the same purpose as the medulla spinalis.

The ganglions which form a chain connected so beautifully together by a double nerve, must be considered to have the same uses, whatever they are, as the ganglions in the human body, being equally composed of a congeries of nerves. These are facts, which if they are allowed to be clearly made out, form an addition to our knowledge, and give confirmation to opinions not before satisfactorily established.

Here I shall conclude the present Lecture, neither Mr. BAUER's time nor my own having admitted of our proceeding farther in this curious and interesting anatomical investigation, which I am not without hope, in the course of another year, of our rendering as complete as microscopical observations admit of, by examining the nervous system of a class of animals in which the existence of a brain has not been ascertained.

Till that is done, I must postpone any physiological remarks connected with the subject, all the facts belonging to it not having been determined.

EXPLANATION OF THE PLATES.

PLATE I.

Fig. 1. A small portion of the human cerebrum in a recent state, which had been immersed in distilled water; magnified five diameters.

Fig. 2. A smaller portion, magnified twenty-five diameters; showing the arrangement of the globules, in straight lines, which pass uninterruptedly across the cortical substance into the medullary.

Fig. 3. A still smaller portion, magnified two hundred diameters, by which means the globules are rendered conspicuous.

Fig. 4. The brain of a tench, of the natural size. The cranium is removed, and the upper surface of the brain exposed to view.

Fig. 5. The under surface of the same brain.

Fig. 6. A horizontal section of the same brain; showing that the nodules have a cortical substance internally, and a medullary on the outer part.

Fig. 7. The cavity in the tench's brain.

Fig. 8. The brain and nerves of the humble bee; magnified ten diameters.

The brain is of a truncated oval form; it gives off the nerves to the eyes and feelers; its internal structure made up of globules. The substance, corresponding in its uses to that of the medulla spinalis, is nodulated on its external surface, connected to the brain by two long chords, which differ from nerves in collapsing soon after being exposed; these I shall call crura cerebri: the lower portion of this nodulated structure corresponds to the medulla spinalis, and, in its internal structure, resembles the brain.

The two nerves that go down, connected at certain distances by small nodules, are different from nerves in being more pulpy, and the nodules themselves are composed of congeries of nerves, similar to the ganglions in the human body, of which there are many excellent representations before the public, which makes it unnecessary to show their structure upon this occasion.

Fig. 9. The brain and nerves of the garden snail; magnified four diameters.

In this animal the brain is made up of two apparently equal portions. As the appearance at the termination of the two large horns resembles eyes, and SWAMMERDAM has attempted to delineate the different parts of the organs: Mr. BAUER has shown the two nerves in different states.

The medulla spinalis forms a larger mass than the brain, but equally made up of two distinct parts. From the upper edge of this mass, there is an azagos branch going directly upward to the muscles of the tongue, beyond which are the glands of the mouth, and the œsophagus cut through.

This nerve, so similar to the recurrent in the human body, only differing in being single, justifies me in having given the name of spinal marrow to the part that gives it off.

Fig. 10. The point of one of the large horns, magnified fifty diameters; to show that the external point of its termination in no respect resembles a cornea, but consists of five bundles of nervous filaments, the terminations of the branches of the nerve.

Plate II.

Fig. 1. The brain, spinal marrow, ganglions, and nerves of the moth of the silk-worm; magnified ten diameters.

Fig. 2. The same parts in a large caterpillar; magnified four diameters.

Fig. 3. The same parts in the lobster; natural size.

Fig. 4. The same parts in the earth worm; magnified two diameters.

Fig. 5. The upper part of the same earth worm; magnified eight diameters.

In Fig. 1, 2, 4, and 5, the ganglions, as well as the brain and spinal marrow, are upon too small a scale to admit of accurate examination, but in a large lobster, the distinction I have made between the brain and spinal marrow and the ganglions, was satisfactorily confirmed, and even in the earth worm it was sufficiently distinct.

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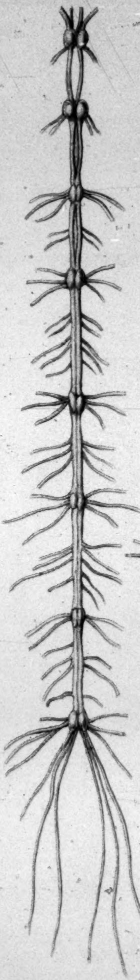

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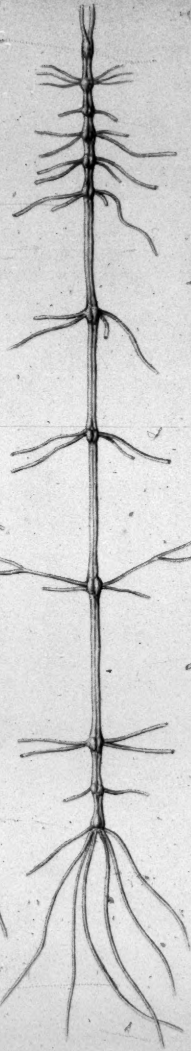

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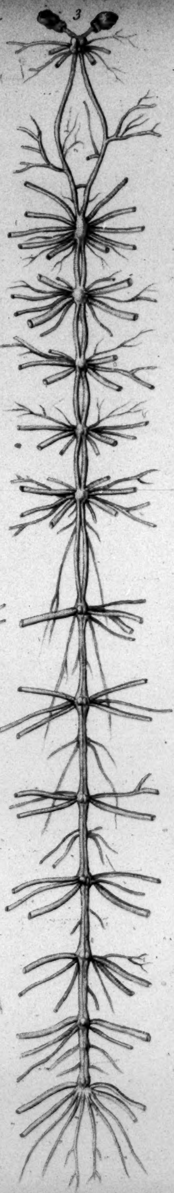

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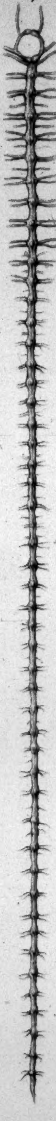

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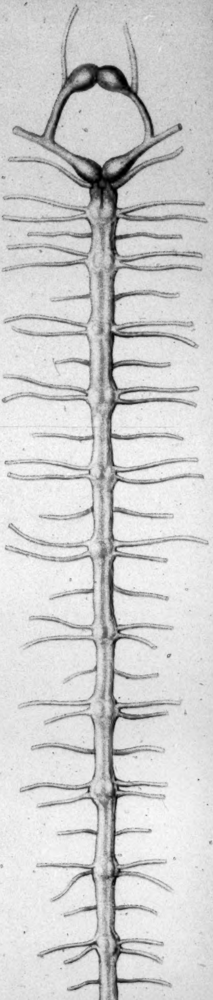

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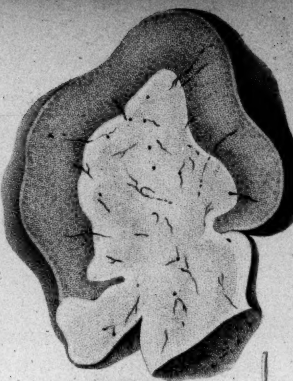

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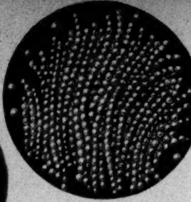

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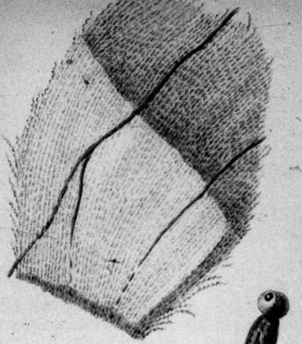

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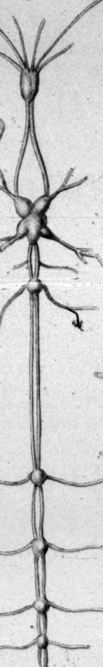

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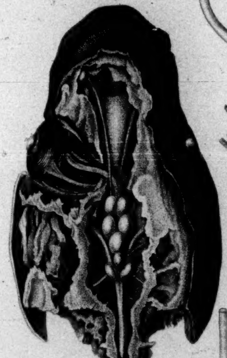

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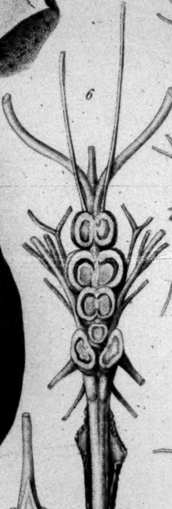

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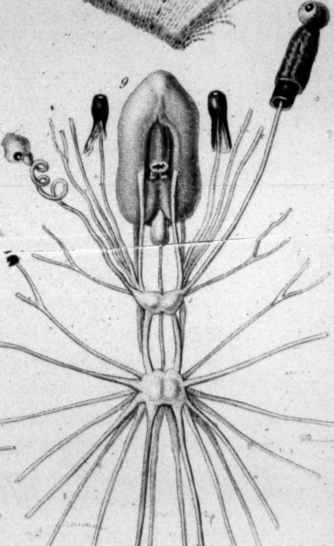

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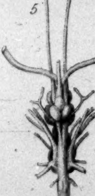

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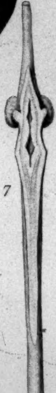

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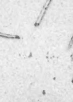

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II. *Some observations on the Migration of Birds. By the late* Edward Jenner, *M. D. F. R. S.; with an Introductory Letter to Sir* Humphry Davy, *Bart. Pres. R. S. By the Rev. G. C.* Jenner.

Read November 27, 1823.

Sir,

It had long been the intention of my late revered Uncle, Dr. Jenner, to lay the accompanying observations on the Migration of Birds, before the Royal Society, as well from inclination, as to redeem a pledge he had given some years ago to that learned Body; but which he was unable to accomplish, in consequence of his extensive correspondence with almost every part of the globe on the interesting subject of Vaccination, which occupied nearly the whole of the time his more immediate professional avocations would allow him to bestow on other objects.

It was my peculiar happiness to accompany Dr. Jenner in most of his investigations of the phenomena of migration; and the Paper I have now the honour of presenting, was left in my hands at the time of his decease.

Had it pleased Providence to have spared him a little longer, he might, probably, have corrected some inaccuracies in the style and order of his Paper, that may now, perhaps, appear conspicuous to the reader, but which I did not conceive myself justified in attempting.

I have the honour to be, Sir,

Your most obedient humble servant,

Stone, near Berkeley,
May 29, 1823.

G. C. JENNER.

To Sir HUMPHRY DAVY, Bart.
President of the Royal Society,
&c. &c. &c.

I.

It is not my intention, in the following pages, to give a general history of the migration of birds. The order in which they appear and disappear, their respective habits, and many other observations, have been given with considerable accuracy by several naturalists, who have paid attention to this very curious subject. It is with a view of representing some facts, hitherto unnoticed, chiefly with respect to the *cause*, which excites the bird, at certain seasons of the year, to quit one country for another, that I communicate the following pages to this learned body.

But before I proceed to state my observations on this head, it may be necessary to adduce some arguments first, in support of the reality of migration, the fact itself not being generally admitted; and, secondly, against the hypothesis of a state of torpor, or what has been called the hibernating system.

In the first place, the ability of birds to take immensely long flights, is proved by the observations of almost every person conversant with the seas. To the many instances already recorded, I shall add the following:

My late nephew, Lieutenant JENNER, on his passage to

Newfoundland, saw, on the 20th of May, the hobby hawk. It came on board, and was secured The day following a swallow came on board. At this time the ship was steering a course direct for that island, and was not within the distance of an hundred leagues of any land. His brother, the Reverend G. C. JENNER, in crossing the Atlantic, observed an owl (of what species he could not precisely ascertain, but he believes it to be the common brown owl) gliding over the ocean with as much apparent ease as if it had been seeking for a mouse among its native fields.* Wild geese have frequently been shot in Newfoundland, whose crops were plentifully stored with maize, or indian corn; consequently, these birds must have taken a pretty bold flight in a short space of time, as no corn of this kind is cultivated within a vast distance of that island. These, however, I do not consider as migrations of any farther consequence, than just to show the powers of the wing.

My ingenious friend and neighbour, the late Reverend NATHANIEL THORNBURY, who had occasionally visited Holland, informed me, that the pigeons about the Hague make a daily marauding excursion, at certain seasons, to the opposite shore of Norfolk, to feed on vetches, a distance of forty leagues. Now, may not this be almost considered as daring a flight as that of the bird which crosses the Atlantic? For it is not at all probable that the shores of this country can be visible to the flock when they set out.

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* Mr. JENNER informs me, that in subsequent voyages he has taken, in the Atlantic, several hundred miles from land, the nuthatch, hoopoe, and snipe; and has often seen small birds of the linnet kind. Of the latter, a large flock came on board, perched on the rigging, appeared very lively, and after adjusting their plumage, and chirping in concert for a few minutes, took their flight in a direction for the Azores.

Again. Is there not something as extraordinary in the pigeon, which can, in a few hours, find out its home, though taken away in a box and totally excluded from the light, to the distance of two hundred miles, as in that bird which quits one shore to seek another, whatever may be the extent of intervening seas? The fact seems to be, that we, the *little lords of the creation*, are too prone to measure the sentient principle in animals by the scale of our own ideas, and thus, unwillingly, allow them to possess faculties which may surpass our own, though peculiarly appropriate to their respective natures; but a little reflection must compel us to confess, that they are endowed with discriminating powers totally unknown to, and for ever unattainable by man. I have no objection to admit the possibility that birds may be overtaken by the cold of winter, and thus be thrown into the situation of other animals which remain torpid at that season; though I must own I never witnessed the fact, nor could I ever obtain evidence on the subject that was to me satisfactory: but as it has been often asserted, may I be allowed to suppose, that some deception might have been practised with the design of misleading those to whom it might seem to have appeared obvious? For far be it from me to insinuate that the subject has been wilfully misrepresented by those naturalists, who have stated it as a fact. Yet how careful should we be in the investigation of all subjects in natural history, which may captivate, by their apparent novelty!

If birds crept into holes and crevices to hibernate, would they not, like quadrupeds, creep out again in a languid state, their fat all absorbed, and their bodies emaciated? We see this fact exemplified in the hedge-hog, one of the most remarkable of our hibernating animals, which retires to its hut at the approach of winter, with vast stores of fat placed in every situation where nature could find room for it. This fat is its only source of nutrition for the winter, which, by the time the sun rouses it to fresh life and activity, is exhausted, and the animal comes forth thin and emaciated. But the case with birds is extremely different. If, on the first day of its appearance, a martin, a swift, or a redstart be examined, it will be found as plump and fleshy as at any season during its stay; it appears also as strong on the wing, and as full of activity at that period as at any other during its abode with us. How the cuckoo, that disappears at so early and so hot a season as the first week in July, can become torpid, is beyond the power of conception.

The apparent incapability of the landrail to perform the task of migration, has often been so strongly adduced as a presumptive argument in favour of the hibernating system, that those who do not admit that of migration, were it to remain unnoticed, might urge it as an objection. It must be admitted, that a superficial examination of the habits of this bird, tends to favour the supposition of its incapacity for so great an exploit, as it often rises from the ground like an half animated lump, and seems, with difficulty, to take a flight of a hundred yards; but let us remark its powers when seriously alarmed. Should it be forced upon the wing by any extraordinary cause, by the pursuit of a hawk, for example, the velocity of its flight, and the rapidity of its evolutions to avoid the common enemy of its race, will at once appear.

This is no very rare exhibition. Necessity here, as in migration, becomes the parent of exertion, which when thus called forth, cannot be shown in a much greater degree by any of the feathered tribe. The moor-hen (which winters with us) gives another instance of what a bird, which appears so much to want activity in its ordinary flights, is capable of performing when exertion is actually required. When pursued by a hawk, and self preservation calls up all its powers, it may be seen to rush up into the air with amazing velocity, almost as high as the eye can reach, then darting down with an equal pace, it often, by such rapid manœuvres, escapes the destructive talons of its swift pursuer.

It is a remarkable fact that the swallow tribe, and probably many other birds which absent themselves at stated periods, should return annually to the same spot to build their nests. The swift, which for nine months has some distant region to roam in, was selected for the purpose of an experiment to ascertain this with precision. At a farm-house in this neighbourhood I procured several swifts, and by taking off two claws from the foot of twelve, I fixed upon them an indelible mark. The year following their nesting places were examined in an evening when they had retired to roost, and there I found several of the marked birds. The second and third year a similar search was made, and did not fail to produce some of those which were marked. I now ceased to make an annual search, but at the expiration of seven years, a cat was seen to bring a bird into the farmer's kitchen, and this also proved to be one of those marked for the experiment.

That the bird, when the stimulus for migration is given, with the choice before it of almost any part of Europe for its annual excursion, should so uniformly not only revisit this island, but even select the same spot for its breeding place, is certainly a wonderful occurrence. But if birds were not instinctively directed to return to their old haunts, should we not find them over crowding some situations, while others would be left desolate? And would not this be the case if the search of food was the object of their migration? However it may be admissible, in one point of view, to consider the bird in its state of migration from this country, as a nearer neighbour than at first might be conceived, if we may be allowed to consider distance, or space, in the instance before us, as governed by the power of progressive motion, of what consequence is it to the swift, which, to use the animated expression of Mr. WHITE, "dashes through the air with the "inconceivable swiftness of a meteor," whether he comes to us from some neighbouring country, or the shores of Africa? The wonder excited by the return of these birds again to their old nesting places, would at once cease, if we could believe what has been asserted by some naturalists, and gained credit with many, namely, that at the time they disappear from us, they submerse themselves in ponds and rivers, and in this situation become torpid. If this idea had not been encouraged and supported by some new hypothesis, I should hardly have thought it necessary to have taken any serious notice of it; but as the matter now stands, I will just state my opinion, why I think it impossible for any birds to be disposed of in this way.

Permit me first to call to your recollection the season of the year at which many of these birds disappear. It happens when they feel no cold blast to benumb them, and when the common food with which they are supported, is distributed through the air in the greatest abundance. At such a time, what can be the inducement to them and their young ones, which have but just began to enjoy the motion of their wings, and play among the sun-beams, to take this dreary plunge? And how is the office of respiration to be performed during the nine months watery residence? The structure of the lungs of birds, differs not essentially from that of quadrupeds, and therefore all communication with the atmosphere being cut off from the first moment of submersion, the possibility of a bird living nine months, or indeed as many minutes under water, appears to be totally irreconcilable with the nature of their structure. I have taken a swift about the 10th of August, which may be considered as the eve of its departure, and plunged it into water: but like the generality of animals which respire atmospheric air, it was dead in two minutes.

The late Doctor Beddoes has thrown out a supposition, that by frequent immersion in water, the association between the movements of the heart and lungs might perhaps be destroyed, and that an animal might be inured to live commodiously for any time under water. As this will probably give new vigour to the languid system of the advocates for the submersion of birds, I think it incumbent upon me to mention it.

Though we frequently see the swallow and the martin sprinkle and splash themselves as they glide over the surfaces of ponds and rivers, yet we never see them dip under for a single moment; indeed a few plunges would so moisten their wings as to prevent their flying, and we should see them occasionally in this disordered state fluttering on the shore. If they went to the sea side, and got beyond the reach of the eye to inure themselves to this element, how could they return, divested as they must be either of the means of swimming or flying? Whoever has observed the common tame duck driven to the necessity of repeatedly diving from the pursuit of a water-dog, must have noticed how exhausted it rises to the surface of the water after a short period of submersion, and how incapable it is of flying, in consequence of the soaking of its wings. The same may be said of birds more in the habit of diving, the grebes and divers. When entangled in a net they soon perish, or when they happen to dive under ice that may chance to overspread a pond; no uncommon place of resort for some of the smaller species of grebes.

I have always been much attached to that faithful animal, the Newfoundland dog, and have often procured from that country those dogs that had been much accustomed to diving, and which had been kept to the practice; yet I never observed that any of them attained by habit the power of remaining under water longer than thirty seconds, and even then, on rising to the surface, they appeared confused. Negroes and other men who have been employed in seeking among sunken rocks the hidden treasures of the deep, are said to have acquired a habit of remaining some minutes under water, but the time was probably measured by a rude guess, and not by a stop watch.

Having thus called the attention of the Society to such statements as give support to the fact of migration, and having also endeavoured to controvert the notion of an hibernating system, I beg to draw their attention to what I conceive to be the true *cause* of migration.

At the coming on of spring we observe our more domestic birds, those that approach our houses, and are most familiar to us, assuming new habits. The voice, gesticulation, and the attachment which the male begins to show to the female, plainly indicate some new agency acting upon the constitution.* This newly excited influence, which so conspicuously alters the habits of our birds at home, is, at the same time, exerting itself abroad upon those which are destined to resort hither. *It is the preparation which nature is making for the production of an offspring by a new arrangement in the structure of the sexual organs, (viz.) the enlargement of the testes in the male, and the ovaria in the female.*

No sooner is the impulse arising from this change sufficiently felt, than the birds are directed to seek a country where they can for a while be better accommodated with succours for their infant brood, than in that from which they depart.†

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* The Rook, among many others, exhibits a familiar instance of the change of voice.

† Birds of the same species that are commonly stationary in this island throughout the year (I say *commonly*, for all, I believe, occasionally migrate), are migrators in other countries. The adult bird might, perhaps, find a subsistence for itself in the country it quits during the incubating season, but the nestling is probably the object nature chiefly holds in view, both with respect to food, and to the temperature of the air in which it is first to feel existence. The one may be unfit or too scanty, and the other too hot or too cold. It is wonderful to see with what peculiar care the parent birds select the food for their young until they are four or five days old. For the most part it is purely animal, but not an atom even of that is suffered to go into the nestling's stomach, that is not perfectly adapted to the tender state of

It is not at the commencement of this enlargement, nor until it is considerably advanced, that the birds are prompted to migrate ; and this is very wisely ordered ; for were they to set off, when first the testes and ovaria begin to grow tumid, they must waste much time here unnecessarily, and indeed arrive at too early a period to find a supply of food. Very little time is lost after their arrival, before they form their connubial alliances.* The business of nesting then begins ; and as a convincing proof that nesting is the chief cause of their errand here, this, and its natural consequences, occupy their attention from the time of their coming to the day of their departure. This is illustrated by the dispatch which some of them make in performing the object of their mission. The cuckoo

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its digestive powers. While the swift is feeding on small beetles that have hard crustaceous wings, and whose habitations are the air, its nestlings are fed in their early state with gnats. The sparrow, a granivorous bird, feeds its young for several days after they are hatched, with the softest insects only, now and then introducing a little coarse sand, smooth on the surface, to inure the stomach, as I suppose, to bear the same kind of substances in a more rugged state, which will shortly be required.

* Should a fatal accident befal either the male or female bird after this alliance is newly formed, no time is lost in unavailing sorrow, nor any great nicety shown in forming a new connection, as the following little history will evince. A pair of magpies began to build their nest in a gentleman's garden at Burbage, in Wiltshire. Disliking their familiarity, he shot one of them from an ambush made for the purpose. The next day there were again a pair going on with the work. One of these was also shot. The loss was not long in repairing, for the day following the pair were again complete, when another fell a victim to the gun. Thus the gentleman went on destroying one of them daily until he had killed seven ; but all to no purpose—the remaining magpie soon found another mate. The nest was finished, and young ones were produced, which were suffered to fly. This is an extraordinary fact.—It seems to show that nature has a reserve of birds in an unconnected state, immediately ready to repair losses. Were the whole to pair at once, the circumjacent country might be insufficient to furnish food for the immense number of young ones that must burst forth at the same time.

finishes this business in a shorter space of time than any other bird, but as he deviates so widely from the common laws of the feathered society, I shall select the swift, as a better example for pointing out the fact. The swift shows himself here about the beginning of May (sometimes a few stragglers appear earlier) and by the beginning of August he has completely reared his young ones, which seldom consist of more than two. At once the old birds and their family take their leave and are seen no more for that season. Now his farther residence cannot be rendered unpleasant by any disagreeable change in the temperature of the air, or from a scarcity of his common food, which at this time abounds in the greatest plenty. This circumstance of the early departure of the swift, without a more apparent cause, seems to have excited much astonishment and perplexity in the mind of that attentive and ingenious naturalist, the late Mr. WHITE, of Selborne. Speaking of the swift, (Letter XXI. page 184), he says, " But in nothing are swifts more singular than in " their early retreat. They retire, as to the main body of " them, by the tenth of August, and sometimes a few days " sooner; and every straggler invariably withdraws by the " twentieth, while their congeners all of them stay till the " beginning of October, many of them all through that " month, and some occasionally to the beginning of November. This early retreat is mysterious and wonderful, since " that time is often the sweetest season of the year. But " what is more extraordinary, they begin to retire still earlier in the most southerly parts of Andalusia, where they " can be no ways influenced by any defect of heat; or as " one might suppose, defect of food. Are they regulated in

" their motions with us by a failure of food, or by a propen-" sity to moulting, or by a disposition to rest after so rapid " a life, or by what? This is one of those incidents in Natural " History that not only baffles our searches, but almost eludes our guesses!" Thus Mr. WHITE.

Now, should the principle I have laid down be admitted, namely, that these birds come here for scarcely any other purpose than to produce an offspring, and retreat when the task is finished, how easily will all circumstances be reconciled? and how little mysterious will those things appear which naturally seemed unaccountable, not only to the amiable author from whom the foregoing passage is taken, but also to others, who have written before on the same subject.

It is somewhat remarkable, that so sagacious a philosopher as the illustrious and learned RAY, who so clearly saw the object of migration in fishes, should not also have been led to a sight of it in birds. After making a very just observation respecting salmons, that quit the sea and ascend up rivers with no other view than to find a place of security for their spawn in the sand; he directly says again, adverting to birds, " What moves them to shift their quarters? You will " say the disagreeableness of the air to the constitution of " their bodies, or want of food."*

The spring migrating birds do not arrive here at first in very large numbers. It may be observed, that in the early part of April a few swallows may be seen; soon after these a few solitary martins, and as the month advances now and then a swift. On the walls of Berkeley Castle, martins build their nests in great numbers. I availed myself of their situ-

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[*] RAY, on the Wisdom of God in the Creation. Part 1., page 128.

ation, and took several of them on the same night, the latter end of May. On dissection, the cause of their gradual and successive migrations appeared obvious, the testes and ovaria being in very different states of progressive forwardness. While one bird presented embryo eggs in the ovarium as large as peas, in another they were found no larger than hemp-seed. These were the extremes; for in the other birds there appeared all the intermediate stages, from the enlargement of the ovaria, sufficient to give the stimulus for migration, to the degree of forwardness just described. The same gradations in the state of the testes of the male corresponded with that of the ovaria in the females. This progressive arrival is not confined to the swallow tribe: all the birds that come early in the spring appear in the same gradual manner. I cannot help observing, that here the wise design of Providence is very conspicuous. Their appearance keeps pace with that of the insects which are to afford them food. If the numbers which flock in upon us in May, were to arrive in April, when only part of them appear, all must be insufficiently supplied, and many of course perish from a want of the needful succours; but by the middle of May, myriads of insects have produced eggs, and great numbers have either brought forth, or matured their progeny; and it may be remarked, there is still a greater increase of insect food by the time the young birds begin to require it. Swallows, on their first coming, feed principally upon gnats. These insects are called forth from their wintry retreats when the air is but moderately heated, 48 degrees of FAHRENHEIT's thermometer being sufficient to put them on the wing. It is in pursuit of them that we see, in cool weather, the swallow incessantly skimming over the surface of ponds and brooks; and their thus early hovering over water has strengthened the idea of their having lately emerged from their watery abode, where they are supposed to have lain dormant during the winter. But they are driven by necessity to feed on the gnat. Like the swift and martin, their more favourite food is a small beetle of the scarabæus kind, which, on dissection, I have found in far greater abundance in their stomachs than any other insects.

The tumid state of the testes and ovaria sometimes comes on prematurely, and in the same manner sometimes subsides. When this happens, swallows and martins desert their nestlings, and leave them to perish in the nest. The economy of the animal seems to be regulated by some external impulse, which leads to a train of consequences. When this change in the testes and ovaria takes place, the bird becomes impelled by a stronger principle, that is, the desire of self preservation. This sometimes happens when they produce a very late hatch. A pair of martins hatched four broods of young ones in the house of a tradesman in this place in the year 1786. The latter brood was hatched in the early part of October. About the middle of the month the old birds went off, and left their young ones, about half fledged, to perish. The pair returned to the nest the 17th of May, 1787, and threw the skeletons out.

Thus scarcely a winter passes but we hear of a nest of robins, hedge-sparrows, and some others of the smaller birds. We have been informed by PENNANT, and it has been noticed also by others, that the cuckoo has been heard to give his song so early as the middle of February, two months sooner than the usual time. The same deviation from the ordinary course of nature, which prematurely occasions the pairing of our domestic birds above-mentioned, proves the stimulus, I conceive, to certain unseasonable migrations, and accounts for the irregularity first noticed. The same argument is of course applicable to the premature appearance of any other migrating birds. The month of March sometimes affords us warm weather for several successive days. At this time I have often seen the snake basking under a hedge. The lizard too, has been invited from his cold retreat; but never could I see the swallow or the martin, although I have taken every opportunity of looking for them during the transient sunshine, and made diligent enquiries of others. At the further advancement of spring, often in April, when, from the long prevalence of north-easterly winds, the weather becomes unseasonably cold, and even frosty, swallows, martins, and other early migrators appear among us. But they soon experience the hardships of an inhospitable reception: the insects that should afford them food being still in a state of torpor in their wintry recesses, and unless called forth by some agreeable change in the air, the unfortunate birds perish for want of food. This I have known happen during an inclement spring, and have picked up starved martins under their nesting places, and willow wrens, which have perished under hedges, through a want of succours.

Unlike the migrating birds that winter with us, of which I shall speak in a subsequent part of this paper, the spring or summer birds do not possess the disposition to change the scene and seek a more genial clime, when this country is so overspread by frost as to deny them their common supplies.

This, I imagine, will admit of an easy explanation. The winter birds require nothing here but food and shelter. Our summer visitors come for more various and important purposes. Had they, like the former birds, been endowed with a disposition to wander on certain changes of the atmosphere, the great design of their migration, as it must have proved fatal to the business of incubation and the rearing of their young, would have been frustrated. It may be worthy of remark, that both the summer and winter migrating birds are, on their arrival here, well received by the domestic natives, and neither create quarrels nor excite fears. The redstart builds its nest in the same tree with the titmouse, and the redwing feeds peaceably in the same meadow with the starling.

I proceed now to make some observations on another kind of migration, directly opposite to the foregoing, namely, the return of the spring migrators to their respective *homes*.

The great disproportion in numbers between those species of birds which quit the country in summer, and those that leave it at the autumnal season, has led naturalists to lose sight of the early migrators, and to confine their reflections on the subject to the late ones only. Hence the common observation, that they are *all* driven off through a failure of food, or a cold temperature of the air. But seeing that many of them disappear in the summer season, when food is placed before them in the greatest plenty, we must seek for some other cause. If we examine what is now going forward in the animal economy, dissection will point out a change in the testes and ovaria, the very opposite to that which took place in the spring. These parts now begin to shrink,* the disposition for raising a farther progeny ceases, and the nuptial knot is dissolved. What inducement have they to stay longer in that country where, I think, it clearly appears their chief object is to multiply their species? This being now effected, they retire to different parts of the globe, doubtless better suited to their general dispositions and wants, when disengaged from parental duties. In many of the migrating species, indeed in the far greater number, the disposition for farther incubation, and the season for their procuring a farther supply of insect food, cease at the same time. It is pretty evident from the habits of the cuckoo and the swift, that quit us in the summer as soon as their nesting is at an end, that swallows, martins, and those birds that disappear in the autumn, would depart at an earlier season, even though their supplies were to continue, if the rearing of their young were perfected. Indeed, as has been before observed, so strong does this propensity now and then appear, that it overcomes even the obligation of rearing their young when hatched late in the season, and they are sometimes left in a callow state to perish in their nests. This premature departure, probably arises from a reverse of that stimulus which occasions the too early migration of the spring birds, as has been noticed in a former part of this essay, namely, a change which takes place in organization.

One of the most singular occurrences in the history of migration, is the mode of departure of the young birds from the country where they were produced. It may be conceived that the bird which had once crossed the Atlantic, or any other ocean, might have something impressed upon it that should prove an inducement to its return; but this cannot be an incitement to the young one. The identical bird, which but a few weeks before burst from the shell, now unerringly finds, without any apparent guide, a track that leads it safely to the place of its destination, perhaps in many instances over the widest oceans.

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* I examined a female cuckoo the first week in July, and found the oviduct shrivelled, and all the eggs disposed of.

It is well known, that those birds which incubate several times in the course of one summer, forsake their first broods when they no longer require their protection: and being now alienated, they cannot, in their parents, find the guides that conduct their course. As swallows and martins congregate* prior to their departure from us, it may be said that their young, though discarded, may mingle with the common flock, and in this particular instance I am ready to admit that it is probable they may do so; but there are many migrating birds that never either associate with swallows and martins, or join together in flocks, as the nightingale, redstart, and indeed the far greater number. As a striking proof that the parent bird cannot possibly be the guide, in one instance at least, we may point out the cuckoo, whose offspring finds a distant shore in perfect safety, although it could never know the parent to whom it was indebted for existence, and though its existence in numberless instances must have taken place even after the departure of the parent. For the old cuckoos invariably leave us early in July, when many of their eggs are yet unhatched in the nests of those small birds to whose fostering care they are entrusted. Compared with quadrupeds, and some other animals, birds may be considered as acquiring the adult state at an early period, and the young bird, at the time of its leaving us, may be looked upon as possessing power equal to the old one in procuring food, velocity of flight, &c. The parent bird, from having lost that stimulus by the subsiding of the testes and ovaria, which urged it to incubation and detained it here, is now reduced to a condition similar to that of its offspring, both falling into the same habits, and remaining in the same state with respect to organization, until the returning calls of nature urge them to quit that country again to which they are *now* about to depart.

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* Swallows and martins congregate on the sunny sides of buildings for the sake of warmth, and not, as it is generally supposed, to hold a kind of consultation previous to their final departure. In the wet summer of 1821, when the air was unusually chilled by the long continued rains, they were observed to assemble, during some intervals of sunshine, for several successive mornings, as early as the middle of July; and in the present year (1822), I remarked the same on some mornings that were unseasonably cold about the middle of August.

II.

Winter Birds of Passage.

> " We have, 'tis hoped, made it pretty evident that summer
> " birds of passage come to and depart from us at certain sea-
> " sons of the year, merely for the sake of a more agreeable
> " degree of warmth, and a greater plenty of food; both which
> " advantages they procure by an alternate change of climate;
> " but the migration of winter birds of passage, and particu-
> " larly of fieldfares and redwings, is much more difficult to
> " be accounted for, there being no such apparent necessity
> " either on the score of food or climate, for their departure
> " from us."
>
> Mr. CATESBY, Phil. Trans. No. 483.

The winter birds of passage, as they are commonly called, begin to take their leave of us about the same time that the spring migrators are taking wing to pay us their annual visit. As the latter appear among us in gradual succession, so in like manner the former disappear. They are both actuated by the same impulse, the former in leaving, and the latter in coming to this country, namely, the enlarged state of the testes and ovarium. As soon as the stimulus becomes sufficiently felt, they quit their homes in quest of a country better suited to their intended purpose than their own.

That a want of food cannot be the inducement, must be obvious to the slightest observer. When the redwing and fieldfare quit this country, it abounds with that food which they prefer to any other; and at this time they are in the finest condition; the redwings often enjoying their plenty by assembling together on trees, and there uniting their feeble voices, make no unpleasant song.*

The winter birds (the females at least) may be said to seek a better accommodation, upon the same principle as the poor woman who quits her cottage for the comforts of a Lying-in Hospital. Here, both herself and suckling are for a while supported in that peculiar way which their situations at that time require. For this reason, conceiving it will tend to lessen confusion, I choose to call this country the *home* of the winter birds (though not natives), and the countries from whence they come, the home of the summer birds, looking upon the latter merely as visitors; and let it be recollected how soon the visits of some of them are paid; for being governed by an unerring principle, they stay to accomplish one great design only, that of rearing their young, and then return.

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* The same thing happens through the winter, whenever the weather has long continued so mild as to allow them plenty of insect food. The starling, and some other birds which have a short note and weak voice, unites with its companions in the spring, and forms a similar concert.

The countries to which many of the winter birds retire not being very far distant, are better known to us than those to which the summer birds migrate; but I must forbear entering into an enquiry upon this subject, as remote from the design of this paper; and indeed it may be thought I have already, in some instances, digressed too widely from my original purpose.

The migration of the winter birds is less distinctly marked than that of the spring migrators. The snipe, the wild-duck, the wood-pigeon, breed here in considerable numbers; the two latter indeed, particularly the wood-pigeon, are so numerous in summer, that we should hardly be reminded of the migration, did they not pour in upon us in such immense flocks in the winter. They are accompanied by the stock-dove, which I have never known to breed here. The home-bred wild-ducks are easily distinguished by the men who attend decoy-pools, by the meanness of their plumage, when compared to the brightness of those birds which come from abroad. The former are taken some weeks earlier than the latter.

The most conspicuous among the winter migrating birds are the redwings and fieldfares. These are regular and uniform in their appearance and disappearance, and I believe never risk the trial of incubation here, at least I never could hear of a single instance. The food of these birds has in the works of every naturalist I have ever had access to, who had written on the subject, been pointed out as the haw, the fruit of the white thorn.*

This is an error that has long wanted a correction, for in open weather they take them in very scanty quantities, and feed on the ground on worms and such insects as they can find. Although repeated examinations of the contents of the stomach have afforded the best proof of this, yet there is scarcely any need of calling in its aid in the present instance, as we may be convinced of the fact, by seeing them in flocks feeding on the ground in open fields and meadows. I do not deny their taking the haw and other vegetable food from the hedges, but they do it in so sparing a way, that I have remarked, that red wings and fieldfares die through hunger during the long continuance of frosty weather, while the haws on the hedges were by no means deficient. The occasional departure of these and some other winter birds during a long continued frost, must be very obvious. The greater number disappear soon after its commencement, if it sets in very severely: some few are always left behind and are soon starved, if not fortunately relieved by a thaw. Those that are driven to this necessitous migration, probably pursue a track that quickly leads them out of the reach of frost. Of these flights I shall produce instances, which render it probable that they are able even to out-strip its course.

The approach of intense frost is often to a certainty made known to us by the appearance of a numerous tribe of water-birds, some of which are rare, and seldom show themselves here on any other occasion. We commonly see them three or four days prior to the setting in of very severe frosty weather. This was manifest at the latter end of the year 1794, at the coming on of the severe season that ensued. In the river Severn, about a mile and a half to the westward of this place, were seen and taken many species of water-birds, that generally confine themselves to the more northern regions. Far more pleasant is it to see during the continuance of hard frost, the return of those birds which had left us at the beginning. These are pleasant omens, and most certainly forebode a thaw. The following example shows how soon they catch the first opportunity of again seeking those countries from which they were so lately driven by necessity. The day preceding the thaw, the frost being then intense, a gentleman who was shooting observed a large flock of fieldfares, birds that are extremely common here in milder weather. They were as much untamed as if no frost had appeared in our island. He had the good fortune to shoot one of them, which was brought to me. I found it as fat and plump, and in every respect in as good condition, as if it had remained here undisturbed, and had found provision in the greatest plenty, though it was without a particle of food in its stomach. Its last meal was digested; and the frost still remaining, it could find no food for its present support. Now it is very obvious that this bird, and its companions, must have taken a long flight, and probably in a very short space of time; for the intense frost, that was of such duration and so severely felt here, extended far into the more southern parts of Europe, beyond which they must have resorted for that plenty of food which gave plumpness to the one I examined, and doubtless to the whole flock, from their appearing so wild and vigorous. It clearly appears, that in their flight they exceeded the progress of the thaw, as the northern birds did that of the frost. This thaw, though it was again succeeded by frost, came on very rapidly, and occasioned, by the sudden melting of the snow, those destructive inundations through the kingdom, that will not readily be forgotten.

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* "The principal food of these birds while with us, is the fruit of the white "thorn, or haws, which hang on our hedges in winter in prodigious plenty."

Phil. Trans. Vol. XLIV. p. 435.

This account of the fieldfare sets the fact of migration, though from an accidental cause, beyond the reach of doubt. There was no support for it here; the ground was deeply covered with snow, and the intense frost, by its long duration, had destroyed every thing that could afford it succour; it must, therefore, have taken a long flight from this country, and returned to it again at the approach of temperate weather.

Having already made so many digressions, I cannot add another without offering an apology; but as there is something so like providential design in the order in which the song birds chaunt out their warblings during a long summers day, I trust the Society will pardon my laying before them the following observations on the subject.

We must observe, that nature never gives one property *only*, to the same individual substance. Through every gradation from the clod we tread upon to the glorious sun which animates the whole terrestrial system, we may find a vast variety of purposes for which the same body was created. If we look on the simplest vegetable, or the reptile it supports, how various, yet how important in the economy of nature, are the offices they are intended to perform! The bird, I have said, is directed to this island at a certain season of the year to produce and rear its young. This appears to be the grand intention which nature has in view; but in consequence of the observation just made, its presence here may answer many secondary purposes; among these I shall notice the following. The beneficent Author of nature seems to spare no pains in cheering the heart of man with every thing that is delightful in the summer season. We may be indulged with the company of these visitors, perhaps, to heighten, by the novelty of their appearance, and pleasing variety of their notes, the native scenes. How sweetly, at the return of spring, do the notes of the cuckoo first burst upon the ear; and what apathy must that soul possess, that does not feel a soft emotion at the song of the nightingale, (surely it must be "fit for treasons, stratagems, and spoils") and how wisely is it contrived that a general stillness should prevail while this heavenly bird is pouring forth its plaintive and melodious strains,—strains that so sweetly accord with the evening hour!—Some of our foreign visitors, it may be said, are inharmonious minstrels, and rather disturb than aid the general concert. In the midst of a soft warm summer's day, when the martin is gently floating on the air, not only pleasing us with the peculiar delicacy of its note, but with the elegance of its meandering; when the blackcap is vying with the goldfinch, and the linnet with the woodlark, a dozen swifts rush from some neighbouring battlement, and set up a most discordant screaming. Yet all is perfect. The interruption is of short duration, and without it, the long continued warbling of the softer singing birds would pall and tire the listening ear with excess of melody, as the exhilarating beams of the sun, were they not at intervals intercepted by clouds, would rob the heart of the gaiety they for a while inspire, and sink it into languor. There is a perfect consistency in the order in which nature seems to have directed the singing birds to fill up the day with their pleasing harmony. To an observer of those divine laws which harmonize the general order of things, there appears a design in the arrangement of this sylvan minstrelsy. It is not in the haunted meadow, nor frequented field, we are to expect the gratification of indulging ourselves in this pleasing speculation to its full extent; we must seek for it in the park, the forest, or some sequestered dell, half enclosed by the coppice or the wood.

First the robin, and not the lark, as has been generally imagined, as soon as twilight has drawn the imperceptible line between night and day, begins his lonely song. How sweetly does this harmonize with the soft dawning of day! He goes on till the twinkling sun-beams begin to tell him his notes no longer accord with the rising scene. Up starts the lark, and with him a variety of sprightly songsters, whose lively notes are in perfect correspondence with the gaiety of the morning. The general warbling continues, with now and then an interruption, for reasons before assigned, by the transient croak of the raven, the screaming of the jay and the swift, or the pert chattering of the daw. The nightingale, unwearied by the vocal exertions of the night, withdraws not proudly by day from his inferiors in song, but joins them in the general harmony. The thrush is wisely placed on the summit of some lofty tree, that its loud and piercing notes may be softened by distance before they reach the ear, while the mellow black-bird seeks the inferior branches. Should the sun, having been eclipsed with a cloud, shine forth with fresh effulgence, how frequently we see the goldfinch perch on some blossomed bough, and hear his song poured forth in a strain peculiarly energetic; much more sonorous and lively now than at any other time; while the sun, full shining on his beautiful plumes, displays his golden wings and crimson crest to charming advantage. The notes of the cuckoo blend with this cheering concert in a perfectly pleasing manner, and, for a short time, are highly grateful to the ear; but, sweet as this singular song is, it would tire by its uniformity, were it not given in so transient a manner. At length, evening advances—the performers gradually retire, and the concert softly dies away. The sun is seen no more. The robin again sets up his twilight song, till the still more serene hour of night sends him to the bower to rest. And now to close the scene in full and perfect harmony, no sooner is the voice of the robin hushed, and night again spreads a gloom over the horizon, than the owl sends forth his slow and solemn tones. They are more than plaintive, and less than melancholy, and tend to inspire the imagination with a train of contemplations well adapted to the serious hour. Thus we see that birds, the subject of my present enquiry, bear no inconsiderable share in harmonizing some of the most beautiful and interesting scenes in nature.

But let me here remark—how ill would the singing of birds agree with the general appearance of winter—the leafless tree,—the snowy mead,—the frozen rivulet! Yet it must be noticed here, that these rigors, in the midst of this dreary season, are sometimes suddenly softened, and a temperate state of the air succeeds. We are then so enlivened by the transition from extreme cold, to a temperature comparatively warm, that we can listen with pleasure to the enfeebled notes of some of the song birds. How admirable the contrivance! There are several birds which have no continued flow of notes, but a kind of chirp only, consisting of some variety of sounds. During a long continued frost, the earth affords many of the feathered tribe so scanty an allowance, that they preserve themselves with difficulty from perishing; a sudden thaw takes place,—plenty at once appears, and every crop is filled. Tis then we see the redwing and starling assemble in large flocks among elms and apple trees, and, by uniting their voices, produce a song not in the least discordant, but, on the contrary, extremely harmonious. At this time the thrush, and even the blackbird, will occasionally afford us a transient song; but it may be observed, that the notes of these birds are rather to be considered as plaintive, than lively. The lark, too, will sometimes mount in the air, beguiled, as it were, by the faint rays of a wintery sun, but his notes are then as poor and feeble as the beams that call him forth. The robin indeed cheers us with his song during the whole of the winter, unless driven off by intense frost, and is the only bird I know, whose notes, at this time, would fully accord with our feelings, so perfectly do they mingle with the surrounding order of things. The goldfinch, were he now to open his full song upon us, would be as appalling as the tones of the owl in the midst of a fine summer's day.

III.

Mr. JOHN HUNTER, my late valued friend, and honoured preceptor, under whose roof I first caught a gleam of that light which so successfully conducted him through the obscure paths of nature, first demonstrated the different sizes of the testes of birds at different seasons of the year. On a farther investigation of this subject, a fact presented itself to me, which may not be unworthy of the attention of this Society, and, as it is in some measure connected with the preceding observations, I have taken the liberty of annexing it.

In those birds that remain but a short time paired with the female, there appears a vast disproportion in the size of the testes, compared with those that live in the connubial state much longer. The cuckoo and the swift point out the fact most obviously. The common brown wren, which remains united with its female from the early part of spring, until the autumn, exhibits testes very far exceeding in size, either those of the cuckoo or the swift. The cuckoo, although a polygamist, may here be considered in the same point of view as the birds that pair. The time which he devotes to the female being so very short, more so indeed by some weeks than even that of the swift, the testes are formed extremely small in proportion to the size of the bird. I never saw them exceed in size the common vetch, while those of the wren were full as large as a common sized garden pea. The medium weight of the cuckoo is about four ounces and a half, that of the wren but little more than three drachms.*

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* Ornithologists might easily have given us the weight of a bird with greater precision, by divesting the stomach of its contents, previous to the bird being

The testes of the swift, which assume a singular oblong shape, somewhat exceed the cuckoo's in bulk, though not so large as those of the wren. I have selected the wren as an example for this comparison, on account of its diminutive size. The testes of all those birds which are capable of producing young more than once in the breeding season, become tumid, as far as I have seen, in the same proportion as those of the wren.

As there are many birds, which, if unmolested, produce but one nest of young ones in the course of the season, it may be asked, why nature should cause as great an enlargement of the testes in these, as those which breed more than once; and why they should exceed in bulk those of the cuckoo or the swift? The answer, I presume, is obvious. Should any ill accident befall the nestlings of the swift when advancing to maturity, the injury would be irreparable, the parent bird being destined to quit the country before another offspring could be reared. The cuckoo is in the same predicament; but the wide dispersion of its young ones, (being placed singly in the nests of other birds), gives them such security as almost to preclude the possibility of their general destruction.* But it is not so with those birds which make a longer stay; should similar accidents befall them, they can repair their losses. Nature, as long as incubation could serve their

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weighed. For example: how very different must the weight of the owl be, which, in its nocturnal flights, had the luck to pick up a mole or two, compared with that which had met with opposite fortune; or of the falcon, that had picked the bones of a leveret, or of the one that was killed with an empty stomach.

* May not this be offered as another reason, why its eggs and young ones are entrusted to the fostering care of so great a variety of birds? It could not have time, during its short stay, to rear so large a progeny; and by no other means could it have placed its numerous brood so much out of the way of danger.

purposes, would keep an accumulation of the proper powers in store, which, in the case of the cuckoo and swift, would be entirely useless.

Whether there be a regular gradation in the size of the testes (that of the bird itself being considered), throughout the whole race, in proportion to the time taken up in pairing, I cannot determine, not having had an opportunity of subjecting the matter to a full investigation. However, I thought the fact already shown of sufficient importance in natural history, to be worthy of communication, as it forms a kind of sequel to Mr. HUNTER's paper on the subject.

With due deference to the late Dr. DARWIN, I am inclined to think that the opinion he set forth respecting the pairing of cuckoos, was taken up hastily, and that the birds which his friend saw were not cuckoos feeding their nestlings, but goat-suckers, whose mode of nesting corresponds with the relation given, and whose appearance might be mistaken for them by one not perfectly conversant with the plumage and the general appearance of cuckoos when on the wing. Is it probable that the cuckoo, which is invariably a polygamist, and never pairs, nests, or incubates in this part of the island, should fall into opposite habits in another part?

To recapitulate the substance of my observations. I have first adduced some arguments in support of migration, the fact itself not being generally admitted by naturalists of celebrity, and also against the hypothesis of a state of torpor, or what may be termed the hybernating system. I have represented that the swallow tribe, and many other birds that absent themselves at stated periods, return annually to the same spot to build their nests; and at the same time that any inference drawn from this fact in support of a state of torpor, would be fallacious upon physiological principles. That certain periodical changes of the testes and ovaria, are the inciting causes of migration. I have stated many facts, hitherto, I believe, unnoticed, chiefly with respect to the *cause* which excites the migrating bird, at certain seasons of the year, to quit one country for another, (viz.) the enlargement of the testes in the male, and ovaria in the female, and the need of a country where they can for a while be better accommodated with succours for their infant brood, than in that from which they depart. It is also attempted to be shown, that their departure from this country is not in consequence of any disagreeable change in the temperature of the air, or from a scarcity of their common food, but the result of the accomplishment of their errand, *i. e.* the incubation, and rearing of their young, and the detumescence of the testes and ovaria. That successive arrivals of migrating birds are attributable to the progressive developement of the generative system in the male and female: that progressive developements are wise provisions of nature; that premature arrivals and departures are frequently to be accounted for on the same principle; that the departure of the spring migrators is owing to a change in the testes and ovaria, the very opposite to that which took place in the spring; that the departure of the young birds is not guided by the parent, but the result of an unknown principle.

In the second part of this paper, some observations are made on the winter birds of passage; that they quit their homes (this country) in spring, in quest of a country better suited to their intended purpose than their own; that they are actuated by the same impulse in quitting this country, that causes the spring birds to come to it, and that want of food cannot be the inducement; that the emigration of the winter birds is less complete than that of the others (the spring migrators); that some species breed here, especially the wild-duck and wood-pigeon; that the redwings and fieldfares are the most regular and uniform in their appearance and disappearance, and most probably never risk the trial of incubation here;* that they quit the country *temporarily* in severe and long continued frost through want of food, and return to it again at the approach of more temperate weather; that the arrival of water-birds forebodes the approach of intense frost, the usual return of the winter-birds, a thaw; that examinations of the latter prove them to have taken long flights before their return, and sets the fact of temporary migration beyond the reach of doubt.

I have then made a digression, and introduced some observations on the singing of birds; and in a third part, given some additional particulars respecting the different sizes of the generative organs of birds, as they appear at different seasons of the year.

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* I must be understood by the word "here," to mean that part of Gloucestershire under my own observation.

III. *On the nature of the acid and saline matters usually existing in the stomachs of animals.* By William Prout, *M. D. F. R. S.*

Read December 11, 1823.

That a free, or at least an unsaturated acid usually exists in the stomachs of animals, and is in some manner connected with the important process of digestion, seems to have been the general opinion of physiologists till the time of Spallanzani. This illustrious philosopher concluded, from his numerous experiments, that the gastric fluids, when in a perfectly natural state, are neither acid nor alkaline. Even Spallanzani, however, admitted that the contents of the stomach are very generally acid; and this accords not only with my own observation, but with that, I believe, of almost every individual who has made any experiments on the subject.

With respect to the nature of this acid, very various opinions have been entertained. Some of the older chemists seem to have considered it as an acid, *sui generis;* by others it was supposed to be the phosphoric, the acetic, the lactic acid,* &c. No less various have been the opinions respecting its origin and use; some supposing that it is derived from the stomach itself, and is essential to the digestive process; others, that it is derived from the food, or is a result of fermentation, &c.; in short, there seems to be no physiological subject so imperfectly understood, or concerning which there has been such a variety of opinions.

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* After I had discovered the principal fact related in this paper, I was surprized to find how nearly Scopoli had come to the same conclusion. He did not indeed come to the conclusion, as far as I can ascertain, that free muriatic acid exists in the stomach, but he advanced the opinion, that the muriatic acid, in union with ammonia, found in such abundance in the stomach of ruminating animals, is secreted by that organ itself. The only account of Scopoli's experiments I have seen is in Johnson's Animal Chemistry, i. 183.

The object of the present communication is to show, that the acid in question is the *muriatic acid*, and that the salts usually met with in the stomach, are the alkaline muriates. As to the origin and use of these principles, as well as the occasional appearance of other acids, &c. in the stomach, I reserve what I have to say on these subjects till a future opportunity, and shall merely remark at present, that the facts now adduced seem to be intimately connected, not only with the physiology and pathology of the digestive process, but with other important animal functions.

Having ascertained the circumstances above mentioned in a general manner, and by means which it would be here unnecessary to detail, an attempt was made to contrive some unexceptionable method by which their truth might not only be satisfactorily demonstrated, but at the same time that the relative quantities of the different principles might be determined: after various attempts, the following processes were adopted for these purposes.

The contents of the stomach of a rabbit, fed on its natural food, were removed immediately after death, and repeatedly digested in cold distilled water till they ceased to impart any thing to that fluid. The whole of these different portions of fluid, which always exhibited strong and decided marks of acidity, were then intimately mixed together, and after being allowed to settle, were divided into four equal portions. 1. The first of these portions was evaporated to dryness in its natural state, and the residuum burnt in a platinum vessel; the saline matter left was then dissolved in distilled water, and the quantity of muriatic acid present determined by nitrate of silver in the usual manner; the proportion of muriatic acid, in union with a *fixed* alkali, was thus determined. 2. Another portion of the original fluid was super-saturated with potash, then evaporated to dryness, and burnt, and the muriatic acid contained in the saline residuum determined as before. In this manner the *total* quantity of muriatic acid present in the fluid was ascertained. 3. A third portion was exactly neutralised with a solution of potash of known strength, and the quantity required for that purpose accurately noticed. This gave the proportion of *free* acid present; and by adding this to the quantity in union with a fixed alkali, as determined above, and subtracting the sum from the *total* quantity of muriatic acid present, the proportion of acid in union with *ammonia*, was estimated. But as a check to this result, the third neutralised portion abovementioned was evaporated to dryness, and the muriate of ammonia expelled by heat, and collected. The quantity of muriatic acid this contained was then determined as before, and was always found to represent nearly the quantity of muriate of ammonia as before estimated; thus proving the general accuracy of the whole experiments beyond a doubt. 4. The remaining fourth portion of the original fluid was reserved for miscellaneous experiments, and particularly for the purpose of ascertaining whether it contained any other acid besides the muriatic. The experiments abovementioned seemed to preclude the possibility of the presence of any destructible acid; and the only known fixed acids likely to be present were the sulphuric and phosphoric; the muriate of barytes, however, neither alone, nor with the addition of ammonia, produced any immediate precipitate,* showing the absence of these two acids in any sensible quantity, and still farther confirming the results as before obtained.

In this manner the three following results, selected from a variety of others of a similar nature, were obtained.

	No. 1	No. 2	No. 3
	grs.	grs.	grs.
Muriatic acid in union with a *fixed alkali*†	·12	·95	1.71
——————*with ammonia*	1·56	·76	·40
————in a *free or unsaturated state*	1·59	2·22	2·72
Total	3·27	3·93	4·83

These results then seem to demonstrate, that free, or at least unsaturated muriatic acid in no small quantity exists in the stomach of these animals during the digestive process; and I have ascertained, in a general manner, that the same is the case in the stomach of the hare, the horse, the calf, and the dog. I have also uniformly found free muriatic acid in great abundance in the acid fluid ejected from the human stomach in severe cases of dyspepsia, as the following examples show. The original quantities of the fluids operated on, of course were various, but for the sake of comparison they are reduced, in the following table, to one pint, or 16 fluid ounces, which quantity, in three instances, (selected from many others) was found to contain of

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* It may be proper to remark, that ammonia, after some time, caused a flocculent precipitate, consisting of the earthy phosphates in union with vegetable and animal matter, and that after combustion, traces of sulphuric acid, the result of that process, were very perceptible. But it is evident, from the experiment related in the text, that neither of these acids previously existed in the original fluid in a free state.

† For the sake of analogy, the chlorine, in union with the basis of the *fixed* alkali, is reduced in this table and the following to the state of muriatic acid.

	No. 1.	No. 2.	No. 3.
	grs.	grs.	grs.
Muriatic acid in union with a *fixed alkali*	12·11	12·40	11·25
———————— *with ammonia**	0· 0	0· 0	5·39
———— in a *free or unsaturated state*	5·13	4·63	4·28
Total	17·24	17·03	20·92

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* I have never in more than one instance, (No. 3, of the above table) been able to detect any sensible quantity of the muriate of ammonia in the fluids ejected from the human stomach; and upon enquiry of Sir ASTLEY COOPER, who was kind enough to furnish me with the fluid for examination, I was informed that the patient was in the habit of frequently taking ammonia as a medicine.

IV. *On the north polar distances of the principal fixed stars. By* JOHN BRINKLEY, *D. D. F. R. S. &c. Andrew's Professor of Astronomy in the University of Dublin.*

Read December 18, 1823.

THE apparent disagreement of the Catalogues of North Polar distances of the fixed stars, as given by different astronomers, has lately excited considerable attention. Many persons may be induced to imagine, that the means of making observations are not in so perfect a state as has been supposed.

The following examination of some important points relative to this subject, will, I hope, be deemed not unworthy of the notice of the Royal Society.

A comparison of the North Polar distances of Mr. POND and Mr. BESSEL, with my own, may give occasion to some useful enquiries. It will give me an opportunity of stating the results of my researches relative to southern motion, to which my catalogues of 1813 and 1823 are, as is known, quite opposed.

In discussing these subjects, I hope I shall be considered as searching after truth, not as handling a useless controversy, than which nothing can be more injurious to science. It will be necessary for me to enter into a considerable detail, I shall therefore briefly state the objects of the following enquiries.

Of the recent Catalogues that have been formed of the principal fixed stars, two, those of Dublin and Greenwich, agree very exactly. That of Mr. BESSEL differs considerably; but the differences are such that they would agree by a modification of the constants of refraction used. This leads me to some considerations respecting the different modes in which my Tables of Refraction, and those of Mr. BESSEL, have been constructed. I do not venture to decide which Catalogue will ultimately be found more correct, that of Dublin, and consequently that of Greenwich, or that of Konigsberg.

Mr. POND, however, does not admit the agreement of the Dublin and Greenwich Catalogues, because we use different refractions, and for comparison, takes my column of North Polar distances, computed by BRADLEY'S refractions.* From the differences then resulting, he infers a flexure of my instrument. But that such reasoning is inconclusive, will, I think, appear from what I shall afterwards state.

In asserting the general agreement of the Catalogues of Dublin and Greenwich, both for 1813 and 1823, I mean, they agree within certain narrow limits. The mean of the differences of the Catalogues of 1813 is only a few tenths of a second. The mean of the differences of the Catalogues of 1823 is still less. It must therefore at first view appear extraordinary, that from the comparison of the two Catalogues of Greenwich a southern motion is deduced, whereas none appears from a comparison of the two Dublin Catalogues; but this is easily explained by an examination of the Catalogues.

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* It ought to be noticed that Mr. POND, in his paper in the First Part of the Philosophical Transactions for 1823, has omitted to state distinctly, that the polar distances he reasons on respecting the flexure of the instrument, &c. are not what I consider as my polar distances. In one Table, indeed, he puts "by BRADLEY'S refractions" at the head. But even here a reader might suppose that they were the North Polar distances as given by me. In the same Table he places BESSEL'S unchanged, by the side of mine changed, and compares them together.

From the weight of external testimony that I shall adduce, I think it will readily be conceded to me, that the southern motion does not exist. It will follow, therefore, that the mean Southern motion must be regarded as an error belonging to one or both of the Greenwich Catalogues of 1813 and 1823. It may be inferred, that the mean error principally belongs to the Catalogue of 1813, as the mean exactness of the Greenwich Catalogue of 1823 may be inferred from its agreement with the Dublin Catalogue of 1823. This is the only way it can be inferred. The observations by reflection only go to prove a relative exactness; for, in consequence of the Pole Star not having been observed at Greenwich by reflection, it was necessary for Mr. POND to assume the latitude of Greenwich, more or less, to accommodate it to the mean error of the Catalogue.

In my researches relative to the Southern motion, I have been able to avail myself of the result of important observations by Dr. BRADLEY, made at Wanstead, in 1728; of zenith observations made in France, in 1740; of Dr. MASKELYNE'S observations at Schehallien, in 1774; of General MUDGE'S observations with the zenith sector, in 1802; and of General LAMBTON'S zenith distances observed in the Mysore, in 1805.

All these observations were made with instruments not inferior to the zenith sector with which BRADLEY so exactly ascertained the quantity of the aberration of light, and it is not necessary for my purpose to suppose them superior.

It has been said, that the Westbury observations of Mr. Pond confirm the Southern motion, as also a few stars observed by Mechain, in the late French measurement. But the irregularities to be found in comparing the Westbury Catalogue with the two Catalogues by the Greenwich mural circle, show that the former cannot be of any use in this enquiry. The few French results that appear to support the Southern motion, are opposed to other results by better instruments.

The Palermo Catalogue, published by M. Piazzi, as containing the correct result of all his observations, when compared with Bradley's Catalogue of 1755, and the two Dublin Catalogues afford a remarkable testimony in favour of the uniformity of the annual variation in declination of the principal stars.

This result of the question of the Southern motion, appears adverse to the opinion advanced by Mr. Pond, relative to the decided superiority of the Greenwich over the Dublin circle. If we are to judge of the instruments by the observations, I am probably right in the opinion I have long entertained, of the unfitness of the Greenwich circle for the accurate investigation of small motions. Whereas I have generally found my instrument consistent in that respect; unless it be said, it has deceived me in regard to the parallax of α Lyræ. This, resting on the authority of the Greenwich instrument, I am not at present disposed to admit. I had intended concluding with some notices respecting Mr. Pond's paper "on the Parallax of α Lyræ," read before the Royal Society on the same day as that relative to the Southern motion; but as that paper requires to be particularly remarked on, I shall here confine myself to the consideration of the North Polar distances, and as connected therewith of the Southern motion.

On the Catalogues of North Polar Distances.

I have placed, in Table I., beside each other, the North Polar distances observed at Greenwich about 1813, and at Dublin about the same time, together with the differences. In like manner have been placed also the Greenwich and Dublin North Polar distances of 1823, together with the differences. An inspection of these will show, except in one or two instances, a very extraordinary agreement. Many of the Polar distances differ by less than 1″; and with the exception of Sirius, in the Catalogue of 1813, the differences are never greater than what might arise from accidental circumstances. The Greenwich Catalogue of 1813 is *less* in its mean quantity than that of Dublin by 0″,47, and the Greenwich Catalogue of 1823 is greater than that of Dublin in its mean quantity by 0″,10. But we are to consider that these Catalogues are computed by different tables of refraction. The constant of refraction $\left(\frac{m-1}{\text{Sin. }1''}\right)$ in BRADLEY's table (that used by Mr. POND) is 57″. In my Table it is 57″,72.

My constant has been determined by the circle and the meteorological instruments used here, and therefore must necessarily be adopted for my observations. When an astronomer has found the constant of refraction by his own instruments, his Catalogue of North Polar distances ought to be formed independently of any other instrument or table of refractions. No partial change can be admitted. Mr. POND, however, as has been mentioned, has done otherwise, and applied BRADLEY's refractions to my Catalogue.*

I particularly regret this circumstance, because it has occasioned my Catalogue to appear to differ more from that of Mr. BESSEL, than it really does. The differences that actually exist are sufficiently difficult to account for. Indeed had Mr. POND also reduced the Catalogue of Mr. BESSEL by the same refraction, the differences would have appeared much better. But this mode of proceeding would not have been less objectionable. From the differences between his own Catalogue and my Catalogue reduced, Mr. POND infers that my telescope is subject to flexure by the quantity of the difference at each zenith distance. Now it must appear a very extraordinary law, and not easily reconcilable to any mechanical principle, that the flexure should be nearly as the tangent of the zenith distance. This it must necessarily be according to his method of changing my North Polar distances.

It is evident, by comparing the two Catalogues, that there is no difference between them but what might arise from unavoidable errors. Had each star been exact to the tenth of a second, still Mr. POND's reasoning would have led him to do the same. He would have reduced them by BRADLEY's refractions, and so made the Catalogues differ. He attributes the differences to flexure. Now he admits that the flexure would be the same at equal distances on each side of the zenith; but it does not appear to have occurred to him that my refractions were determined by observations of circumpolar stars to the north of the zenith by the same instrument, and that therefore they must be *exactly* in error by the quantity of flexure; and so when applied to stars south of the zenith, must *exactly* compensate for the effects of flexure.* Mr. POND did not perceive that what he took away with one hand, he ought to have restored with the other, and so left my Catalogues as he found them.

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* It may be said, that in a Paper in the Transactions of the Royal Irish Academy, about eight or nine years ago, I changed *my own* North Polar distances for the purpose of comparison. But the circumstances are entirely dissimilar. I have always referred to, and always used, the North Polar distances computed by my own refractions.

Mr. BESSEL, in his comparison of my polar distances with his own, does not change mine to adopt his own refractions. He knew I had determined my own with my own instruments.

It is difficult to say how far the difference of our constants of refraction may be occasioned by a discordance in the meteorological instruments. This should be enquired into. It is still more difficult to imagine a difference in the mean refractions at the two places.

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* The manner in which the telescope and circle are attached in the Dublin instrument, appears to preclude all probability of flexure in the telescope. Indeed it does not appear a matter of much difficulty where the telescope and circle are combined together, as in the Dublin and Greenwich instruments, to guard against a flexure in the telescope. If talents such as those of Mr. RAMSDEN and Mr. TROUGHTON have been unable to provide against the flexure of the telescope, it appears to me quite useless to expect exactness in the other parts of the instruments. Therefore, it might be considered as almost a waste of time to endeavour to overcome the difficulties I should have to encounter here by observing by reflection. The difficulties in general would be greater than at Greenwich; and, above all, among the few clear days that occur, very few could be found sufficiently calm to observe by reflection.

Mr. POND had a motive for pursuing this mode of observation which does not exist here; he had no other method of determining his zenith point. I do not consider the observations by reflection necessary for my own satisfaction, but if they be for that of others, I should not object to undertake them.

In whatever way the subject is considered, the coincidence of the Greenwich and Dublin Catalogues speaks in the strongest manner for the excellence of the divisions of both instruments.

This coincidence will, if I mistake not, appear in a stronger point of view, by deducing the co-latitude of Greenwich from applying the zenith distance of each star, as observed by reflection at Greenwich, to the Polar distance of the same star as given in the Dublin Catalogue for 1823. The results are given in Table 2. The mean of the 30 stars is 38° 31′ 20″,8, or two tenths of a second less than that assumed by Mr. POND, and four tenths greater than that found by Mr. BESSEL, *from Dr.* BRADLEY's *observations.*

The difficulty that has arisen from the comparison of the Greenwich and Dublin Catalogues with that of Mr. BESSEL, is now to be considered. In this also, there will, I think, be nothing found adverse to that degree of accuracy, which is supposed to belong to modern instruments and modern observations.

It will readily appear, that the differences between the Dublin Catalogue and that of Mr. BESSEL, are equivalent to a change in the constant of refraction of about one second. If, in computing the Dublin observations, we increase my constant of refraction by half a second, and in computing the Konigsberg observations, we decrease Mr. BESSEL's constant of refraction by half a second, the Catalogues will be found to agree sufficiently.

It is not necessary to search for other causes till we are assured this is not the true one. The investigation of the exact constant of refraction will be found one of great difficulty, if we consider the nature of it. Mr. BESSEL and I have proceeded by different methods, and, in some respects, my method appears more likely to lead to an accurate result.

Mr. BESSEL's object is to obtain a formula that shall embrace all elevations from the zenith to the horizon, and, therefore, he necessarily assumes a law of variation of density in the atmosphere.

In my investigation, I only consider zenith distances not greater than about 75° or 76°, where no sensible effect is produced from our ignorance of the law of variation of density. Let us consider the advantages and disadvantages of each method.

Mr. BESSEL* supposes the equation of density to be

$$\rho=(\rho)e^{-\frac{g-l}{g-l}as}$$

ρ being the density at the height, *as* & (ρ) that at the surface, a being the radius of the earth, and l the height of an uniform atmosphere. He proposes to find g, so that the formula of refraction deduced may satisfy the observations. He has therefore two unknown quantities, g, and the constant of refraction, k.

When we consider the irregularities of refraction at low altitudes, and the number of observations required to make those irregularities disappear, it may be thought that the problem is unnecessarily involved by requiring the investigation of two unknown quantities, and, under the circumstances of the case, there is reason to suppose that the observations may be satisfied within certain small limits, by assigning values to k, even differing 1″, by making corresponding changes in g, so that the problem partakes too much of the nature of an indeterminate one. Thus the advantage apparently gained by large refractions, is lost by attendant inconveniencies.

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* Astronom. Fundament. p. 28.

In my investigation, there is only one unknown quantity, but then I have much smaller quantities to work with.

Theory gives as far as about 76°, whatever be the law of variation of density in the atmosphere.

* The mean refraction $(r) = k \tan. z - \frac{k\,l \tan. z}{a \cos.^2 z}$ &c. (1), z being the zenith distance.

By a table of refractions, or by the pole star, and a star or stars more remote, k is easily obtained nearly. Then if the true value be $k + dk$

$$dr = dk \tan. z \;(2)$$ sufficiently exact.

Let A and B be the observed zenith distances of a circumpolar star, (considering B negative when south of the zenith) above and below the pole, R & R′ the refractions *exactly* computed by the formula (1), k being the approximate constant of refraction.

Then by (2)

$$\text{Co-lat.} = \frac{A + B + R + R' + dk \tan. A + dk \tan. B}{2}$$

Hence, if C represent the mean co-latitude thus determined by circumpolar stars remote from the pole, and N that by stars near the pole, we obtain an equation of the form

$$C + m\,dk = N + n\,dk$$

$$\text{and } dk = \frac{N - C}{m - n}$$

In this investigation the Z. D. of the stars remote from the Pole, should not be greater, when below the Pole, than about

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* Transactions of the Royal Irish Academy, Vol. 12.

76° or 77°, and not less than about 69° or 70°. Let us suppose it in its mean quantity at about 73°, and then the value of *m* will be about $\frac{\tan. 73^\circ}{2} = 1{,}6$, and for the stars near the Pole, the mean value of *n* about 0,7. Hence $m - n$ is less than unity, and, consequently, the error of the constant of refraction is greater than the error of N — C. Now I think it will be conceded to me, that it requires the exactest instruments and exactest observations to determine the quantity N — C certain to half a second. A greater number of stars can be used for determining C than for N, but then the greater zenith distances will probably occasion C to be more inexact than N. In C, the irregularities of refraction, and in N, the errors of division, have most influence.

In a series of observations in which I am at present engaged, for determining anew the constant of refraction, I use for N the Pole star only, and I lessen the effect of the errors of division, that may be apprehended, by being enabled to observe the Pole star in all parts of its daily course.

I shall not anticipate here the probable result of these observations.

The object of the above, is not to examine whether the constant of refraction has been determined with greater exactness at Dublin or at Konigsberg, but only to endeavour to show that the uncertainty, which exists, cannot be considered in any manner adverse to the received opinion respecting the exactness of modern observations and modern instruments.*

Before leaving this subject, I may be permitted to make a few remarks relative to one circumstance, that Mr. BESSEL relies on a good deal, as proving the exactness of his refractions, viz. that they give the obliquity of the ecliptic at the Winter and Summer solstices the same. Whereas other Tables of refraction give the obliquity in Winter less than in Summer.

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* A catalogue of Polar distances necessarily exhibits, for low stars, the error of the constant of refraction as it were considerably magnified.

We have lately commenced here to observe the zenith distance of the sun every day at noon, on which it can be seen.

I had formerly been unwilling to observe the sun with the circle, except at the solstices, as I considered the heat likely to derange the instrument for my observations relative to parallax.

The Dublin circle, in one respect, is well adapted for observing the sun. By observing a few minutes before and after noon, four observations give me the zenith distance of the centre, independently of the semi-diameter, or correction for collimation.

Observations on eighty-seven days have been obtained during the last year. The manner in which I have used them, is, I believe, somewhat new. With the declination in the Nautical Almanac, and the meridional zenith distance deduced from the observation, I obtain the latitude of the Observatory. I assume, that the declination in the Nautical Almanac is only erroneous by an error in the longitude (L) of the sun, and obliquity of the ecliptic (O). Then, for each day, I have the lat. = Z. D. observed ± decli. $+ m\,d\mathrm{L} + n\,d\mathrm{O} + p\,dk$. From the nature of the Solar Tables it may be assumed, and the assumption is exact enough for my enquiry, that dL only arises from an error in the place of the equinox.

The mean of the latitudes thus found during the year, will be affected by an error $s\,dL + t\,dO + v\,dk$, in which the coefficients s and t are so small, that the effects of dL and dO will be insensible. Thus the eighty-seven observations give my latitude $= 53° 23' 12'',39 + 0,04\,dL + 0,21\,dO + 1,42\,dk$.

By circumpolar stars remote from the Pole

Co-lat. $= 36° 36' 47'',15 + 1,62\,dk$,

making the sum $= 90°$,

we deduce $dk = 0'',15 - 0,01\,dL - 0,07\,dO$.

This small value of dk appears to confirm the accuracy of the constant k that I had used. But if I relied on this I should deceive myself; for on examining the series of latitudes deduced, it is evident that this coincidence arises from the circumstance of more observations having been made while the sun was on the north side of the equator, than while on the south. The latitudes deduced show clearly, that had more observations been made nearer the winter solstice than the summer, the value of dk would have been much more considerable.

This contradictory result, and some other circumstances that appear on an examination of the latitudes deduced, seem to point out that some new equation is required to be applied for the solar refraction. At least, that no conclusion can be drawn as to the exactness of a table of refractions, from its giving the obliquity of the ecliptic the same at the two solstices.

On the Southern Motion.

In Table III. are given the North Polar distances of forty-six principal stars, from recent observations with the circle of the Observatory of T. C. Dublin, and also the North Polar distances of the same stars from the Catalogue of 1813* reduced to 1823.†

The column of differences shows that there are none greater than what may be attributed to accidental circumstances, especially when it is considered that the Catalogue of 1813 was formed from a small number of observations of each star. The mean difference = + 0″,2, whereas the mean difference of Mr. Pond's Catalogues = + 1″,1. In this then our instruments are at variance. The discordance appears much more striking if we examine the differences that exist as to certain stars. It is from these, unless I am much mistaken, I shall be enabled to show the greater exactness of the Dublin instrument. But it may be useful to add a few remarks respecting the *mean difference,* to show there are reasons for supposing a constant error, which, being allowed for, would considerably reduce the above mean difference of 1″,1.

By Table I. it appears, the mean difference between the Dublin Catalogue of 1813, and the Greenwich Standard Catalogue of 1813 = + 0″,47. The mean difference between the Catalogues of 1823 = — 0″,10. Now, supposing for a moment these differences are errors belonging to the Greenwich instrument; that is, the Catalogue of 1813 is in defect = 0″,47, and the Catalogue of 1823 is 0″,10 in excess. Here then the mean southern motion would be reduced by 0″,6, and there would remain only 0″,5 to be accounted for, half of which *might* be accounted for by a circumstance to be mentioned presently.

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* Trans. R. I. Academy, Vol. 12, p. 69.

† To the Catalogue which was published in the Journal of Science, October, 1822, have been since added several stars, viz. α Persei, Rigel, α Hydræ, 2 α Libræ, α Herculis, and α Pegasi. In that Catalogue, the mean difference from that of 1813 was exactly 0″,0.

The error I suppose in the Catalogue of 1823 is so small, that the observations by reflection cannot be adduced to controvert it; this, as I have mentioned, could not even be done had the supposed error been much greater, in consequence of the latitude having been assumed. The observations by reflection have only shown the consistency of the North Polar distances, not their absolute exactness.

The N. P. D. of the Pole Star in the Standard Catalogue of 1813

	°	′	″
	= 1	41	21,6
10 years variation = 19″,457 × 10		— 3	14,6
Predicted, 1823	1	38	7,0
Observed, 1823	1	38	7,5
			0,5 South.*

Mr. Pond has not remarked this apparent southern motion of the Pole star, which is so nearly equal to the sum of the mean differences of the Greenwich and Dublin Catalogues of 1813 and 1823. It is highly probable, that this apparent southern motion of the Pole star has arisen from small errors in determining the place of the Pole star at each period.

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* Excepting error, if any, from lunar nutation.

It will be remarked in the Catalogue, Table III., that the Pole star, as determined with the Dublin instrument at the two epochs, agrees exactly.

Predicted, 1823, 1° 38′ 7″,3
Observed, 1 38 7,3

This is a mean between Mr. POND's observed and predicted places.

A circumstance above alluded to is of some importance. In Table IV. will be found the annual variations as found by Mr. POND, by myself, and by Mr. BESSEL. Mine are between those of the other two, but nearer to Mr. POND's than to Mr. BESSEL's. The effect of this would be, as to mine, to reduce the mean southern motion of Mr. POND, about a quarter of a second; but if Mr. BESSEL's annual variations be adopted, they would, in conjunction with the above supposition relative to the Pole star, intirely take away the mean southern motion of Mr. POND's Catalogue.

It will be found, I conceive, difficult, in forming a Catalogue of stars by a mural circle, to avoid a small constant error, and if the Greenwich observations of the Pole star be consulted from the beginning, we shall find enough to induce us to suppose, that such errors may exist in one or both of the Greenwich Catalogues of 1813 and 1823.

In respect to the annual variations, I shall not venture to give an opinion whether Mr. POND's or Mr. BESSEL's be more exact. I shall only state that mine, which are generally between the two, were formed, as will easily be seen on examination, by a careful comparison of my Catalogue of 1823, with the Catalogue* deduced by Mr. BESSEL from Dr. BRADLEY's observations.

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* Astron. Fundament, p. 138, &c.

The above remarks, relative to the mean difference of Catalogues, have been adduced only because I hope they will be found to contain some useful illustration on this subject.

The proofs I shall now bring of the non-existence of a southern motion, are derived from comparing, in years remote from each other, the places of particular stars, supposed by Mr. POND to have a considerable southern motion, with others supposed to have none, or only a very small southern motion. Whatever doubt may arise when we reason on such small quantities as the mean difference, none can occur with respect to several particular stars that have been supposed to have a great southern motion.

The conclusion that follows is, that there is no southern motion similar to what Mr. POND has deduced. There may be certain stars of which the proper motions are not uniform. In some stars these may have a tendency to diminish, and in others to increase, but nothing of this kind is as yet certainly known. Perhaps, hereafter, it may be confirmed that the proper motion of Procyon is increasing.

(I) The stars α Cassiopeæ and γ Ursæ Majoris, are particularly considered by Mr. POND. According to him, α Cassiopeæ appears to have a considerable southern motion relatively to γ Ursæ Majoris.

It is a somewhat singular circumstance, that Dr. BRADLEY observed, with great care, at Wanstead, in 1727 and 1728, the difference of declination between these two stars. It is worth while to quote his own words.*

" But as it may be of some use to future astronomers to " know what were the mean differences of declination, at a " given time, between some stars that lie nearly opposite to

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* Phil. Trans. Vol. 45. Old Abridg. Vol. 10, p. 51.

"one another in right ascension, and not far from either of "the colures, I shall set down the result of the comparison "of a few that differ so little in declination, that I could "determine the quantity of that difference with great certainty." He then states, that the *mean* difference of declination was 10′ 28″,1, on March 27, (old stile) 1727. This, reduced to January 1, 1727, new stile, is 10′ 38″,4.

The declinations of these stars in 1755, reduced from Dr. BRADLEY's observations with the Greenwich quadrant, by Mr. BESSEL,* are

α Cassiopeæ	55° 11′ 23″,7
γ Ursæ Maj.	55 3 24,4
Difference	7 59,3

Dr. MASKELYNE observed these stars at Schehallien, 1774. The observations† of the zenith sector, reduced to January 1, 1774, by the usual equations, give

	Z. D. α Cassiop.		Z. D. γ Ursæ Maj.
Oct. 2	1° 22′ 45″,5	Oct. 14	1° 43′ 22″,2
3	43,5	15	25,4
5	45,7	18	23,4
24	46,4	22	22,3
Mean	1 22 45,3	Mean	1 43 23,3
			1 22 45,3
		Diff. decl.	20 38,0
		Diff. ref.	+0,4
			20 38,4

M. PIAZZI,‡ Palermo.
Declination, 1800.

γ Ursæ	54° 48′ 23″,0
α Cassiop.	55 26 17,6
Diff.	37 54,6

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* Astron. Fund. pp. 140, 208.

† Phil. Trans. Vol. 65.

‡ M. PIAZZI's Great Catalogue, "Panormi, 1814."

Hence this Table

		Observed Difference of Declination.	Variation in 10 Years.	At	Reduced to 1780.
		′ ″	′ ″		′ ″
1727	Dr. BRADLEY, Wanstead.	+10 38,4	6 39,2	1741	6 39,0
1755	Dr. BRADLEY, Greenwich.	— 7 59,3	6 39,5	1764	6 39,4
1774	Dr. MASKELYNE, Schehallien.	—20 38,4	6 38,6	1787	6 38,6
1800	M, PIAZZI, Palermo.	—37 54,6	6 38,4	1812	6 38,5
1823	Dr. BRINKLEY, Dublin.	—53 11,0			

The last column is deduced from the fourth, by computing from* the secular variation of annual precession in diff. decl. Table III. = + 0″,067 — 0″,029 = + 0″,038.

The mean of the last column is 6′ 38″,9, the same as that deduced by comparing the Greenwich observations of 1755, with the Dublin of 1813.

The variations in the last column agree so nearly, that there cannot be a doubt that the apparent motions of declination of these stars have been uniform for upwards of ninety years.

(II.) The observations made in France with a sector, in 1739 and 1740,† appear to have been exact, by comparing the amplitudes of the same arc determined by different stars.

The lunar nutation was then unknown; but if we correct the observations for this, and solar nutation, we may then deduce the differences of declination of certain stars, and compare them with later observations.

According to Mr. POND, Capella has a considerable southern motion relatively to η Ursæ Majoris, viz. at the rate of 1″,9 in ten years, at 1818 greater than at 1784.

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* The secular variation is here and elsewhere given retrospective.

† La Merid. Verif. p. lxxxiii., &c.

(I.) Capella and η Ursæ Majoris observed at Paris.

Z.D. Jan. 1, 1740.

Capella	3° 8′ 28″,6 S.	η Ursæ Maj.	1° 47′ 7″,7. N.
Sol. and lunar nut.	— 5 ,7		— 7 ,6
	3 8 22 ,9		1 47 0 ,1 N.
			3 8 22 ,9 S.
		Diff. decl.	4 55 23 ,0*

	Greenwich, 1755, decl.		Palermo, 1800 decl.		Dublin, 1823, N.P.D.
Capella .	45° 43′ 4″,8		45° 46′ 37″,5		44° 11′ 36″,2
η Ursæ Maj.	50 32 39 ,0		50 18 59 ,2		39 47 59 ,9
Diff.	4 49 34 ,2		4 32 21 ,7		4 23 36 ,3
Paris, 1740	4 55 23 ,0		4 49 34 ,2		4 32 21 ,7
(15 years)	5 48 ,8	(45 y.)	17 12 ,5	(23 y.)	8 45 ,4

The secular variation of annual precession in diff. of declination for these two stars is +,622 + ,153 = +,675. Vide Table III.

Hence,

Rate at 1747	in ten years	3′ 52″,5	reduced to 1780	3′ 50″,3
at 1777		3 49 ,4		3 49 ,2
at 1812		3 48 ,4		3 50 ,5

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* The result from the observations at Bourges is 4° 55′ 22″,8. This close agreement must be accidental. But the observations in general may be considered as exact. It is worth while mentioning here the latitude of the Royal Observatory, at Paris, as deduced by comparing the Dublin north polar distances 1823, with the zenith distances of three stars observed at Paris, 1739 and 1740,

Capella gives lat. = 48° 50′ 14″,0
η Ursæ Maj. - - - - - 13 ,1
γ Draconis - - - - - 17 ,4
Mean 48 50 14 ,8

This differs so little from 48° 50′ 14″, the lat. according to the latest determination, that it shows also we can calculate the motions of these stars pretty exactly for eighty years.

This is as great a coincidence, allowing for unavoidable errors, as could be expected from the most uniform variation. The difference that exists between the 2nd and 3rd, is contrary to a southern motion

(2) γ Draconis and α Cygni,

	Dunkirk, 1740, Z. D.	Greenwich, 1755, decl.	Palermo, 1800, decl.	Dublin, 1823, N.P.D.
γ Draconis	0° 29′ 47″,0 N	51° 31′ 40″,6	51° 31′ 4″,5	38° 29′ 10″,3
α Cygni	- 6 40 12 ,8 S	44 24 56 ,7	44 34 19 ,8	45 20 52 ,0
Diff.	7 9 59 ,8	7 6 43 ,9	6 56 44 ,7	6 51 41 ,7
		7 9 59 ,8	7 6 43 ,9	6 56 44 ,7
		(15 y.) 3 15 ,9	(45 y.) 9 59 ,2	(23 y.) 5 3 ,0

The secular variation of annual precession in diff. of decl. = +, 202 —, 227 = —, 025.

Hence,

	in ten years	reduced to 1780
Rate at 1747	2′ 10″,6	2′ 10″,7
at 1777	2 13 ,1	2 13 ,1
at 1812	2 11 ,7	2 11 ,6

The coincidence here is not so great as before, but there is nothing the least in favour of a southern motion in α Cygni.

(III.) The observations* made by the late General MUDGE, with the zenith sector, in 1802, appear to concur in evidence against the southern motion, by a comparison of the place of Capella with those of γ and η Ursæ Maj. and γ Draconis.

The instrument with which these observations were made, and the care† used in making them, entitle them to great weight.

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[*] Philosophical Transactions, 1803.

[†] It is necessary to remark, that the computations, as given in the Philosophical Transactions, were not made with the same care as the observations. This explains the different results which appear here. For Capella, the lunar nutation appears to have been applied the wrong way.

(1) Mean zenith distance, January 1, 1802.

	Dunnose.	Arbury Hill.	Greenwich.
γ Ursæ Maj.	4° 10′ 37″,8 N	2° 34′ 16″,8 N	3° 19′ 6″,0 N
Capella . .	4 50 21 ,7 S	6 26 41 ,4 S	5 41 51 ,0 S
	9 0 59 ,5	9 0 58 ,2	9 0 57 ,0
	Mean of the three	= 9 0 58 ,2	

	Greenwich, 1755, decl.		Mean from Z. Sect. 1802.		Dublin, Jan. 1, 1823.
Capella . .	45° 43′ 4″,8	Diff.	9° 0′ 58″,2		44° 11′ 36″,2
γ Ursæ Maj.	55 3 24 ,4		9 20 19 ,6		35 19 15 ,1
	9 20 19 ,6	(47 y.)	19 21 ,4		8 52 21 ,1
					9 0 58 ,2
				(21 y.)	8 37 ,1

The secular variation of annual precession = + 0″,622 − 0,029 = + ,593.

Hence,

Rate at 1778	in ten years	4′ 7″,1	reduced to 1780	4′ 7″,0
at 1813		4 6 ,2		4 8 ,1

This discordance, contrary to the southern motion, may be safely attributed to the errors of observation.

(2) If we compare η Ursæ Majoris and Capella,

	Dunnose.	Clifton.	Arbury Hill.	Greenwich.
η Ursæ Maj.	0° 18′ 45″,5 S	3° 9′ 9″,2 S	1° 55′ 6″,2 S	1° 10′ 17″,8 S
Capella	4 50 21 ,7 S	7 40 42 ,7 S	6 26 41 ,4 S	5 41 51 ,0 S
	4 31 36 ,2	4 31 33 ,5	4 31 35 ,2	4 31 33 ,2
The Mean is	4 31 34 ,5			

We may now refer back to (II.) (1), and instead of the Palermo diff. of declination, insert this, and we shall obtain,

Rate at 1747 for ten years, gives rate for ten years at 1780	3′ 49″,9
1778—————————————— 1780	3 49 ,5
1813—————————————— 1780	3 49 ,9

This, therefore, shows a uniform variation.

(3) Capella and γ Draconis.

	Dunnose.	Clifton.	Arbury Hill.	Greenwich.
γ Draconis	0° 53′ 56″,7 N	1° 56′ 26″,9 S	0° 42′ 22″,9 S	0° 2′ 24″,6 N
Capella .	4 50 21 ,7 S	7 40 42 ,7 S	6 26 41 ,4 S	5 41 51 ,0 S
	5 44 18 ,4	5 44 15 ,8	5 44 18 ,5	5 44 15 ,6
	Mean difference	5 44 17 ,1		

	Greenwich, 1755, decl.		From Z. Sector. Mean diff.		Dublin. 1823, N.P.D.
Capella - -	45° 43′ 4″,8		- - - -		44° 11′ 36″,2
γ Draconis - -	51 31 40 ,6		- - - -		38 29 10 ,3
Diff.	5 48 35 ,8		5 44 17 ,1		5 42 25 ,9
			5 48 35 ,0		5 44 17 ,1
		(47 y.)	4 18 ,7	(21 y.)	1 51 ,2

The secular variation of annual precession in diff. of decl. $= +,622 + ,202 = + ,824$.

Hence,

	in ten years	reduced to 1780
Rate at 1778	55″,0	54″,9
at 1813	52 ,0	54 ,7

a result supporting a uniform variation.

It may not be without its use in this enquiry, to show what the latitude of Greenwich comes out independently on the mural circle, viz. by reducing the zenith sector observations of 1802 to 1823 (applying an uniform variation, viz. that deduced from the variations in 1789, by allowing the changes in precession) and then computing from the north polar distances for 1823, determined by the Dublin observations.

Mean Z. D Greenwich, Jan. 1, 1802.

	γ Draconis.	γ Ursæ Maj.	η Ursæ Maj.	Capella.
	0° 2′ 24″,6 N	3° 19′ 6″,0 N	1° 10′ 17″,8 S	5° 41′ 51″,0 S
Reduction to 1823	— 14 ,7	— 7 0 ,0	+ 6 21″,8	— 1 35 ,3
	0 2 9 ,9	3 12 6 ,0	1 16 39 ,6	5 40 15 ,7
Dublin, N. P. D.	38 29 10 ,3	35 19 15 ,1	39 47 59 ,9	44 11 36 ,2
Co-lat. Greenw.	38 31 20 ,2	38 31 21 ,1	38 31 20 ,3	38 31 20 ,5

The mean of these gives the latitude of Greenwich 51° 28′ 39″,5, or one-tenth of a second less than what Mr. BESSEL found from his admirable investigations on Dr. BRADLEY's observations. He strongly contends for the exactness of 51° 28′ 39″,6, which is 0″,6 more than that recently assumed by Mr. POND.

(IV.) Two stars in which Mr. POND finds a great southern motion are γ and α Pegasi.

These two stars were observed by General LAMBTON, at his station of Dodagoontah, in the Mysore, at the same time that α Serpentis was also observed.* For α Serpentis, Mr. POND finds none, or a very small, southern motion.

We have hence an opportunity of comparing the relative changes of N. Polar distance of these stars and α Serpentis.

An examination of the observations in the Asiatic Transactions will show, that for stars so near the zenith, much reliance may be had on the results of the observations.†

(1) γ Pegasi and α Serpentis.

	Greenwich. 1755, decl.		Dodagoontah. 1805, Z. D.		Dublin, 1823, N.P.D.
α Serpentis	7° 12′ 48″,6		5° 56′ 59″,6 S		83° 0′ 36″,6
γ Pegasi -	13 49 14 ,1		1 6 4 ,2 N		75 48 0 ,4
Diff.	6 36 25 ,5		7 3 3 ,8		7 12 36 ,2
			6 36 25 ,5		7 3 3 ,8
		(50 y.)	26 38 ,3	(18 y.)	9 32 ,4

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* Asiatic Transactions, Vol. 10. p. 359.

† It may be said, that the observations made at Dodagoontah were not exact, judging by the irregularities exhibited by the latitude found by different stars. But these were owing to the errors of the catalogue of declination which General LAMBTON possessed. If the zenith distances be reduced, and applied to either of the Dublin Catalogues of N. Polar Distances, the latitudes by each star will be found to agree.

Secular variation of annual precession in diff. decl. = + ,349 + ,013 = + ,362.

Hence,

$$\left.\begin{array}{r}\text{Rate at 1780}\\ \text{at 1814}\end{array}\right\}\text{in ten years}\left\{\begin{array}{l}5'\ 19'',6\\ 5\ \ 18\ ,0\end{array}\right\}\text{reduced to 1780}\left\{\begin{array}{l}5'\ 19'',6\\ 5\ \ 19\ ,2\end{array}\right.$$

Quantities so nearly equal, prove the uniform variation of the diff. of N. P. D. of these stars.

(2) α Pegasi and α Serpentis.

	Greenwich. 1755 decl.	Dodagoontah. 1805, Z. D.	Dublin. 1823, N.P.D.
α Serpentis	7° 12′ 48″,6	5° 56′ 59″,6	83° 0′ 36″,6
α Pegasi -	13 53 29 ,7	1 9 37 ,8	75 44 41 ,3
	6 40 41 ,1	7 6 37 ,4	7 15 55 ,3
		6 40 41 ,4	7 6 37 ,4
		(509) 25 56 ,3	(189) 9 17 ,9

Secular variation of annual precession in diff. decl. = + ,349 − ,116 = + ,233.

Hence,

$$\left.\begin{array}{r}\text{Rate at 1780}\\ \text{at 1814}\end{array}\right\}\text{in ten years}\left\{\begin{array}{l}5'\ 11'',3\\ 5\ \ \ 9\ \ 9\end{array}\right\}\text{reduced to 1780}\left\{\begin{array}{l}5'\ 11'',3\\ 5\ \ 10\ ,7\end{array}\right.$$

From these small differences we cannot conclude a southern motion in these stars when compared with α Serpentis. Mr. Pond's observations made it, in both γ and α Pegasi, upwards of 2″.

(V.) Sirius has, according to Mr. Pond, a greater southern motion than any other star, amounting to 3″,4 for ten years, between 1813 and 1823, compared with its rate for ten years at 1784.

This star, in these latitudes, is far from the zenith, on which account, the result of the observations of M. Piazzi, at Palermo, will be of considerable authority in estimating the value of observations made at Greenwich, and in Dublin.

	N.P.D. Sirius.
The Cat. of M. Piazzi, gives, Jan. 1, 1800	106° 27′ 6″,2
Red. 13 y. (ann. var. 4″,40)	+ 57 ,2
Jan. 1, 1813	106 28 3 ,4
Red. 10 y. (ann. var. 4″,44)	44 ,4
	106 28 47 ,8

Hence,

	1813.	1823.
Computed from Palermo Cat.	106° 28′ 3″,4	106° 28′ 47″,8
Greenwich Cat.	0 ,7	48 ,7
Dublin Cat.	4 ,3	48 ,4

There can therefore be little doubt, that the apparent southern motion of this star at Greenwich, has arisen principally from an error in the result of the Greenwich observations of 1813.

(VI.) Several of the stars of M. Piazzi's Catalogue have been already referred to in this enquiry. It is right to remark, also, the general agreement of the Dublin Catalogue of 1813, and the N. P. D. distances for 1813, deduced from M. Piazzi's Catalogue, taking the annual variations, (reduced to 1806) that were obtained by a comparison of Bradley's Catalogue of 1755, by Bessel, with the Dublin Catalogue of 1823. These variations are given in Table V, column 5.

In this Table, in Column 1, will be found how much the respective stars of the Dublin Catalogue of 1813 are north or south of their places so computed (predicted) from the Palermo Catalogue.

It is evident here is no southern motion, the mean of all the differences is 0″,1 north, a remarkable confirmation of the exactness of the annual variations used.

Column 2, of Table V., shows how much the observed places, 1823, are north or south of their places, computed from the Catalogue of 1813. These results are mentioned before, and are only placed here to be seen at one view with the rest.

It has been supposed, that Mr. POND's Westbury observations afford a confirmation of the southern motion. Column 3, of Table V., shows how much the observed places, at Greenwich, 1813, are north or south of the places predicted from the Westbury Catalogue.

Column 4 contains Mr. POND's differences between his Catalogue, 1823, and the places predicted from his Catalogue, 1813. A comparison of Columns 3 and 4 will show, that the Westbury Catalogue is, in many instances, so irregularly at variance with the Greenwich Catalogues of 1813 and 1823, that no conclusion whatever can be deduced from it.

In the Conn. des Tems. 1809, p. 458, are given declinations of four stars observed by MECHAIN, with the repeating circle of BORDA, which, at first sight, may appear to support the southern motion.

The first of these stars is Capella, N.P.D. 1800, 44°13′18″,0. The zenith distance of this star, observed at Greenwich by General MUDGE with the zenith sector, and reduced to 1800, is 5° 41′ 51″,0 + 9″,2. Hence the co-lat. of Greenwich = 38° 31′ 17″,8. Therefore, either the zenith sector, or BORDA's circle, must have been in error; and had we not proof of the exactness of the sector, we could scarcely hesitate between the two instruments.

Two of the four stars above mentioned are β Tauri and Pollux. Mechain's declination gives the predicted N. P. D. of β Tauri, conformable to Mr. Pond's southern motion, who makes it only 0″,7, a quantity evidently too small to found an argument on; besides, Mechain's places of this star, deduced at Paris and Montjoy, differ by 1″,8.

Pollux also gives a southern motion, but Mr. Pond, finds the southern motion of this star only 0″,2. Also Mechain's results as to Pollux are very discordant.

Mechain's declination of Sirius also seems to support the southern motion, but in this it is opposed by that of Piazzi.

I shall conclude by mentioning a result recently obtained, that shows, in a remarkable manner, that the Dublin circle has been consistent with itself from the beginning, and has suffered no derangement.

From 1809 to 1823, inclusive, thirteen summer solstices have been observed with the circle, for which, observations on eighty seven days have been made. I have investigated from these, the maximum of *lunar nutation*, and found it = 9″,60, which happens to be exactly what I have hitherto used for the Sun. I am induced, however, to give more weight to my result from the stars, viz. 9″,26*. The difference is less than could have been expected from solar observations. It puts beyond all doubt the permanent state of the instrument. Had any circumstances taken place similar to those, of whatever nature they may be, by which the Greenwich instrument has shown so great a southern motion in certain stars, they must have given a very erroneous quantity of lunar nutation.

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* Philosophical Transactions, 1821.

POSTSCRIPT.

Since the above was written, the kindness of a friend has communicated to me, by letter, Mr. POND's Paper, read in June last, and which has appeared in the Second Part of the Transactions recently published: the volume itself has not reached me, and therefore I have not seen the Tables. I find Mr. POND has referred to the Palermo Catalogue, as contained in the Philosophical Transactions, 1806. That Catalogue has been long rejected by the author. The improved places of the principal stars, as given in the great Catalogue, are those to which I have referred. This explanation appears necessary.

The exact Catalogue was first published, probably about 1807, as the Conn. des Tems. 1809, p. 458, which was published in 1807, contains the principal stars agreeing with the great Catalogue very nearly. The observations were therefore made prior to 1807. Indeed it is probable both Catalogues were founded on nearly the same observations. I beg leave to refer here to Mr. BESSEL's "Astron. Fundam. p. 297 and 298," for some remarks relative to the improved Catalogue of M. PIAZZI.

Mr. POND states, that unless the southern motion be admitted, the Greenwich observations of 1813 will appear very erroneous, and those of Dublin still more so. As I am unacquainted with the arguments by which he supports this opinion, I cannot reply to them. But I think quite the contrary, as far as regards my observations, will appear from the preceding pages. The southern motion will change every thing in my results, as well as in those of other astronomers; whereas, without it, every thing is consistent; and I cannot but feel considerable satisfaction in the conviction, that, sidereal astronomy is a more certain science than it is represented to be in the last communication of Mr. Pond to the Royal Society.

Observatory, Trinity College, Dublin,
Dec. 6, 1823.

TABLE I.

	Greenwich Catalogue. N. P. D. 1813.	Dublin Catalogue. N. P. D. 1813.	Diff.	Greenwich Catalogue. N. P. D. 1823.	Dublin Catalogue. N. P. D. 1823.	Diff.
	° ′ ″	° ′ ″		° ′ ″	° ′ ″	″
Polaris	1 41 21,6	1 41 21,8	+0,2	1 38 7,5	1 38 7,3	—0,2
β Ursæ Min.	15 4 49,0	15 4 49,4	+0,4	15 7 15,7	15 7 16,7	+1,0
β Cephei	20 15 30,6	20 15 31,4	+0,8	20 12 54,0	20 12 54,4	+0,4
α Ursæ Maj.	27 14 31,5	27 14 30,9	—0,6	27 17 43,7	27 17 44,0	+0,3
α Cephei	28 12 12,5	28 12 13,9	+1,4	28 9 42,8	28 9 42,7	—0,1
α Cassiopeæ	34 29 22,7	34 29 22,6	—0,1	34 26 5,7	34 26 4,1	—1,6
γ Ursæ Maj.	35 15 55,3	35 15 56,2	+0,9	35 19 14,8	35 19 15,1	+0,3
γ Draconis	38 29 3,7	38 29 3,7	0,0	38 29 10,5	38 29 10,3	—0,2
η Ursæ Maj.	39 44 57,9	39 44 58,4	+0,5	39 47 59,4	39 47 59,9	+0,5
α Persei	40 48 52,6	40 48 51,4	—1,2	40 46 39,2	40 46 37,9	—1,3
Capella	44 12 20,5	44 12 20,7	+0,2	44 11 36,8	44 11 36,2	—0,6
α Cygni	45 22 57,0	45 22 58,3	+1,3	45 20 52,4	45 20 52,0	—0,4
α Lyræ	51 23 0,5	51 23 0,8	+0,3	51 22 31,2	51 22 30,8	—0,4
Castor	57 42 46,7	57 42 47,5	+0,8	57 43 59,0	57 43 58,8	—0,2
Pollux	61 31 56,4	61 31 56,1	—0,3	61 33 17,0	61 33 17,2	+0,2
β Tauri	61 33 43,7	61 33 44,2	+0,5	61 33 6,5	61 33 6,7	+0,2
α Andromedæ	61 56 29,6	61 56 30,3	+0,7	61 53 12,5	61 53 12,0	—0,5
α Cor. Bor.	62 38 55,4	62 38 55,5	+0,1	62 41 0,6	62 41 0,3	—0,3
α Arietis	67 25 36,5	67 25 36,8	+0,3	67 22 44,4	67 22 43,7	—0,7
Arcturus	69 50 19,0	69 50 19,3	+0,3	69 53 29,2	69 53 29,6	+0,4
Aldebaran	73 52 35,4	73 52 36,0	+0,6	73 51 17,7	73 51 17,6	—0,1
β Leonis	74 22 57,3	74 22 56,4	—0,9	74 26 18,1	74 26 17,9	—0,2
α Herculis	75 23 14,0	75 23 14,6	+0,6	75 24 0,1	75 24 0,5	+0,4
α Pegasi	75 47 51,6	75 47 52,8	+1,2	75 44 41,8	75 44 41,3	—0,5
γ Pegasi	75 51 21,0	75 51 21,2	+0,2	75 48 2,4	75 48 0,4	—2,0
Regulus	77 7 22,7	77 7 23,1	+0,4	77 10 15,6	77 10 16,7	+1,1
α Ophiuchi	77 17 39,1	77 17 40,5	+1,4	77 18 10,6	77 18 10,5	—0,1
γ Aquilæ	79 50 0,6	79 50 1,3	+0,7		79 48 37,9	
α	81 36 58,8	81 36 59,8	+1,0	81 35 29,5	81 35 28,9	—0,6
α Orionis	82 38 15,7	82 38 15,9	+0,2	82 38 4,2	82 38 4,0	—0,2
α Serpentis	82 58 39,3	82 58 38,8	—0,5	83 0 36,6	83 0 36,6	0,0
β Aquilæ	84 3 4,1	84 3 5,2	+1,1		84 1 38,9	
Procyon	84 18 14,4	84 18 15,3	+0,9	84 19 43,3	84 19 43,0	—0,3
α Ceti	86 39 0,7	86 39 2,0	+1,3	86 36 36,8	86 36 36,2	—0,6
α Aquarii	91 13 21,6	91 13 21,7	+0,1	91 10 31,4	91 10 30,3	—1,1
α Hydræ	97 51 11,3	97 51 11,0	—0,3	97 53 44,5	97 53 44,2	—0,3
Rigel	98 25 33,8	98 25 34,3	+0,5	98 24 48,4	98 24 48,1	—0,3
Spica Virginis	100 10 51,3	100 10 51,3	0,0	100 14 0,7	100 14 2,0	+1,3
1 α Capricorni	103 4 35,4	103 4 36,1	+0,7	103 2 49,6	103 2 49,7	+0,1
2	103 6 52,3	103 6 52,0	—0,3	103 5 6,6	103 5 7,0	+0,4
2 α Libræ	105 15 22,7	105 15 22,6	—0,1	105 17 56,3	105 17 58,3	+2,0
Sirius	106 28 0,7	106 28 4,3	+3,6	106 28 48,7	106 28 48,4	—0,3
Antares	116 0 16,6	116 0 16,8	+0,2	116 1 44,1	116 1 44,1	0,0

TABLE II.

	N. P. D. Dublin. Jan. 1, 1822.			Zenith Distance. Greenwich. Jan. 1, 1822.			Co-Latitude of Greenwich.		
	°	′	″	°	′	″	°	′	″
β Ursæ Min.	15	7	1,9	23	24	20,0	38	31	21,9
β Cephei	20	13	9,9	18	18	11,3			21,2
α Ursæ Maj.	27	17	24,7	11	13	56,6			21,3
α Cephei	28	9	57,7	9	21	23,1			20,8
α Cassiopeæ	34	26	24,0	4	4	55,0			19,0
Capella	44	11	40,5	5	40	20,4			20,1
α Cygni	45	21	4,4	6	49	44,1			20,3
α Lyræ	51	21	33,8	12	50	13,0			20,8
Castor	57	43	51,5	19	12	31,0			20,5
Pollux	61	33	9,0	23	1	47,8			21,2
β Tauri	61	33	10,3	23	1	49,3			21,0
α Andromedæ	61	53	31,9	23	22	11,6			20,3
α Cor. Bor.	62	39	48,1	24	8	27,1			21,0
α Arietis	67	23	1,1	28	51	40,8			20,3
Arcturus	67	53	10,5	31	21	49,2			21,3
Aldebaran	73	51	25,5	35	20	4,5			21,0
β Leonis	74	25	57,9	42	54	37,1			20,8
α Herculis	75	23	55,9	36	52	34,6			21,3
α Pegasi	75	45	0,5	37	13	40,1			20,4
Regulus	77	9	59,3	38	38	37,5			21,8
α Ophiuchi	77	18	7,5	38	46	46,8			20,7
α Aquilæ	81	35	38,0	43	4	17,6			20,4
α Orionis	82	38	5,2	44	6	44,6			20,6
α Serpentis	83	0	24,9	44	29	3,7			21,2
Procyon	84	19	17,0	45	48	13,6			20,7
α Aquarii	91	11	22,0	52	39	27,7			19,8
α Hydræ	97	52	58,5	59	22	8,4			20,5
Spica Virg.	100	3	43,0	61	32	20,8			22,2
Sirius	106	28	43,9	67	57	23,3			20,6
Antares	116	1	35,6	77	30	14,4			21,2

TABLE III.

Annual Variation. 1818.	Secular Variation. 1718. = 1818.		From recent Observations, Dublin Cat. for 1823.	Computed from Cat. 1813.	Diff.
″	″		° ′ ″	″	″
— 20,077	— 0,013	γ Pegasi	75 48 0,4	0,4	0,0
19,866	— 0,067	α Cassiop.	34 26 4,1	3,9	+ 0,2
19,452	- -	Polaris	1 38 7,3	7,3	0,0
17,382	— 0,240	α Arietis	67 22 43,7	42,9	+ 0,8
14,536	— 0,316	α Ceti	86 36 36,2	36,7	— 0,5
13,430	— 0,456	α Persei	40 46 37,9	37,1	+ 0,8
7,889	— 0,458	Aldebaran	73 51 17,6	17,1	+ 0,5
4,507	— 0,622	Capella	44 11 36,2	35,6	+ 0,5
4,690	— 0,411	Rigel	98 24 48,1	47,4	+ 0,7
3,749	— 0,540	β Tauri	61 33 6,7	6,7	0,0
— 1,299	— 0,473	α Orionis.	82 38 4,0	3,0	+ 1,0
+ 4,444	— 0,380	Sirius	106 28 48,4	48,7	— 0,3
7,157	— 0,527	Castor	57 43 58,8	59,1	— 0,3
8,687	— 0,422	Procyon	84 19 43,1	42,2	+ 0,9
8,054	— 0,491	Pollux	61 33 17,2	16,6	+ 0,6
15,230	— 0,273	α Hydræ	97 53 44,2	43,3	+ 0,9
17,281	— 0,233	Regulus	77 10 16,7	15,9	+ 0,8
19,122	— 0,160	β Urs. Maj.	32 40 16,1	15,9	+ 0,2
19,272	— 0,160	α	27 17 44,0	43,6	+ 0,4
20,049	— 0,036	β Leonis	74 26 17,9	16,9	+ 1,0
20,001	— 0,029	γ Urs. Maj.	35 19 15,2	16,2	— 1,0
19,700	+ 0,084	ε	33 4 38,2	39,1	— 0,9
18,989	+ 0,153	Spica Virg.	100 14 2,0	1,2	+ 0,8
18,960	+ 0,122	ζ Ursæ Maj.	34 8 50,4	52,3	— 1,9
18,173	+ 0,153	η	39 47 59,9	0,1	— 0,2
18,984	+ 0,216	Arcturus	69 53 29,6	29,2	+ 0,4
15,335	+ 0,313	2 α Libræ	105 17 58,3	55,9	+ 2,4
14,765	— 0,029	β Ursæ Min.	15 7 16,7	17,1	— 0,4
12,449	+ 0,295	α Cor. Bor.	62 41 0,3	0,0	+ 0,3
11,740	+ 0,349	α Serpentis	83 0 36,6	36,2	+ 0,4
8,580	+ 0,484	Antares	116 1 44,1	42,6	+ 1,5
4,577	+ 0,387	α Herculis	75 24 0,5	0,4	+ 0,1
3,086	+ 0,400	α Ophiuchi	77 18 10,6	11,4	— 0,8
+ 0,689	+ 0,202	γ Draconis	38 29 10,3	10,6	— 0,3
— 2,993	+ 0,291	α Lyræ	51 22 30,8	30,8	— 0,0
8,326	+ 0,376	γ Aquilæ	79 48 37,9	38,1	— 0,2
9,044	+ 0,384	α	81 35 28,9	29,4	— 0,5
8,555	+ 0,369	β	84 1 38,9	39,7	— 0,8
10,634	+ 0,411	1 α Capric.	103 2 49,7	49,7	0,0
10,669	+ 0,411	2	103 5 7,0	5,3	+ 1,7
12,585	+ 0,227	α Cygni	45 20 52,0	52,5	— 0,5
15,037	+ 0,131	α Cephei	28 9 42,7	43,5	— 0,8
15,656	+ 0,064	β Cephei	20 12 54,4	54,8	— 0,4
17,244	+ 0,227	α Aquarii	91 10 30,3	29,3	+ 1,0
19,285	+ 0,116	α Pegasi	75 44 41,3	40,0	+ 1,3
19,932	— 0,004	α Andromedæ	61 53 12,0	11,0	+ 1,0

TABLE IV.

	Ann. Var. 1820. Mr. POND.	Ann. Var. 1820. Dr. BRINKLEY.	Ann. Var. 1820. Mr. BESSEL.
	″	″	″
γ Pegasi	— 20,09	— 20,08	— 20,03
α Cassiopeæ	19,85	19,87	—
α Arietis	17,40	17,38	17,35
α Ceti	14,59	14,53	14,49
α Persei	13,41	13,43	—
Aldebaran	7,92	7,88	7,86
Capella	4,54	4,50	4,48
Rigel	4,74	4,69	4,66
β Tauri	3,80	3,74	3,71
α Orionis	— 1,36	— 1,29	— 1,27
Sirius.	+ 4,41	+ 4,45	+ 4,48
Castor	7,12	7,16	7,19
Procyon	8,63	8,69	8,74
Pollux	8,02	8,06	8,09
α Hydræ	15,19	15,23	15,27
Regulus	17,23	17,29	17,31
α Ursæ Maj.	19,26	19,27	—
β Leonis	20,04	20,05	20,08
γ Ursæ Maj.	19,98	20,00	—
Spica Virg.	18,94	18,99	19,03
η Ursæ Maj.	18,15	18,17	—
Arcturus	18,97	18,98	19,01
1 α Libræ	15,30	—	15,40
2 α Libræ	15,32	15,33	15,37
β Ursæ Min.	14,74	14,76	—
α Cor. Bor.	12,45	12,44	12,48
α Serpentis	11,72	11,73	11,79
Antares	8,59	8,58	8,65
α Herculis	4,57	4,57	4,61
α Ophiuchi	3,08	3 08	3,12
γ Draconis	+ 0,67	+ 0,68	—
α Lyræ	— 3,02	— 3,00	— 2,96
γ Aquilæ	8,34	8,33	8,29
α Aquilæ	9,06	9,05	9,00
β Aquilæ	8,56	8,56	8,49
1 Capricorni	10,66	10,64	10,58
2 Capricorni	10,68	10,68	10,61
α Cygni	12,63	12,59	12,56
α Cephei	15,07	15,04	—
β Cephei	15,68	15,66	—
α Aquarii	17,27	17,25	17,20
α Pegasi	19,32	19,28	19,26
α Andromedæ	— 19,95	— 19,93	19,91

TABLE V.

	Diff. of Palermo Cat. 1800, and Dublin, 1813.	Diff. of Dublin Cat. 1813, and Dublin, 1823.	Diff. of Westbury Cat. 1800, and Greenwich, 1813.	Diff. of Greenwich Cat. 1813, and Greenwich, 1823.	Annual Variations, 1806.
	″	″	″	″	″
γ Pegasi	1,2 N	0,1 N	5,2 S	2,3 S	— 20,080
α Cassiopeæ	1,4 N	0,1 S		1,5 S	19,875
α Arietis	0,3 N	0,8 S	2,4 S	1,8 S	17,412
α Ceti	0,3 N	0,5 N	0,0	2,0 S	14,579
α Persei	0,2 N	0,8 S		0,9 S	13,494
Aldebaran	1,3 S	0,5 S	3,4 S	1,5 S	7,944
Capella	2,6 N	0,5 S	1,4 S	1,7 S	4,582
Rigel	0,4 N	0,7 S	0,8 N	2,0 S	4,739
β Tauri	0,7 N	0,0	0,2 N	0,8 S	3,814
α Orionis	1,5 N	1,0 S	2,0 S	2,1 S	— 1,356
Sirius	0,9 S	0,3 N	1,7 S	3,4 S	+ 4,399
Castor	0,0	0,3 N	0,4 S	0,9 S	7,114
Procyon	1,5 S	0,8 S	0,8 S	2,9 S	8,636
Pollux	0,5 N	0,6 S	0,5 N	0,2 S	7,996
α Hydræ	1,0 N	0,9 S	0,8 S	1,3 S	15,191
Regulus	0,8 S	0,8 S	4,6 S	0,6 S	17,254
β Ursæ Maj.	0,9 S	0,1 S			19,104
α	1,0 N	0,4 S		0,5 N	19,254
β Leonis	0,5 S	1,0 S	4,4 S	0,4 S	20,046
γ Ursæ Maj.	0,8 N	1,1 N		0,4 N	19,998
ι	1,7 N	0,8 N			19,710
Spica Virginis	0,2 S	0,8 S	1,4 S	0,0	19,008
ζ Ursæ Maj.	1,8 S	1,9 N			18,974
η	1,1 S	0,2 N		0,2 N	18,192
Arcturus	0,5 S	0,4 S	4,6 S	0,4 S	19,010
2 α Libræ	1,3 N	2,4 S		0,6 S	15,376
β Ursæ Min.	2,1 N	0,4 N		0,7 N	14,762
α Cor. Bor.	1,2 S	0,3 S	0,3 S	0,5 S	12,485
α Serpentis	0,7 N	0,4 S	4,1 S	0,1 S	11,782
Antares	1,6 N	1,5 S		1,6 S	8,645
α Herculis	2,2 S	0,1 S		0,4 S	4,627
α Ophiuchi	2,3 S	0,8 N	4,6 S	0,9 S	3,134
γ Draconis	1,1 N	0,3 N	0,8 S	0,2 S	+ 0,713
α Lyræ	0,1 S	0,0	3,2 S	0,8 S	— 2,959
γ Aquilæ	0,3 S	0,2 N			8,281
α	2,0 S	0,5 N	4,5 S	1,2 S	8,999
β	1,0 S	0,8 N			8,510
1 α Capricorni	2,2 S	0,1 N		1,0 S	10,586
2	0,1 N	1,7 S	2,6 S	1,0 S	10,620
α Cygni	1,3 S	0,5 N	3,4 S	1,6 S	12,558
α Cephei	2,0 N	0,8 N		0,6 S	15,022
β	2,4 N	0,5 N		0,2 S	15,647
α Aquarii	0,6 N	1,0 S	0,8 S	2,5 S	17,219
α Pegasi	0,4 S	1,4 S	4,3 S	3,3 S	19,268
α Andromedæ	1,6 N	1,0 S	1,1 N	1,8 S	19,933

V. *On the figure requisite to maintain the equilibrium of a homogeneous fluid mass that revolves upon an axis.* By James Ivory, *A. M. F. R. S.*

Read December 18, 1824.

The theory of the figure of the earth, as delivered in the *Philosophiæ Naturalis Principia Mathematica*, is liable to some objections. In determining the ratio of the axes, the illustrious author assumes that the terrestrial meridian is an ellipse, having the greatest diameter in the plane of the equator. M'Laurin afterwards proved, by a most elegant synthetic process of reasoning, that a homogeneous fluid body, possessed of such a figure as Newton supposed, will fulfil all the conditions of equilibrium arising from the attraction of the particles, and a centrifugal force of rotation. In this manner the assumption of Newton was verified; but the theory was still left imperfect, since it is necessary to determine, by a direct investigation, all the figures of a fluid mass that are consistent with the laws of equilibrium, rather than to show that the same laws will be fulfilled in particular instances. We are indebted to Legendre for the first demonstration that a homogeneous fluid body, revolving about an axis, cannot be in equilibrio by the attraction of its particles, unless it have the figure of an oblate elliptical spheroid. The researches of Legendre were rendered more general by Laplace, who gave a complete theory of the figure of the planets, distinguished by that depth and elegance which is so much admired in all his writings. It is assumed, however by the eminent geometers we have mentioned, that the figure of the fluid mass is but little different from a sphere which is a restriction not essential to the problem, but introduced for the sake of overcoming some of the difficulties of the investigation. In the following Paper, the figure of a homogeneous fluid body, that revolves about an axis, and is in equilibrio by the attraction of its particles, is deduced by a direct analysis in which no arbitrary supposition is admitted.

1. It is necessary to begin this research, with laying down some general properties of the attractions of bodies; and we cannot better accomplish this end, than by considering the function, which is the sum of all the molecules of a body divided by their respective distances from the attracted point. Conceive any material body to be divided into an indefinitely great number of molecules, one of which is represented by dm; and having drawn three planes intersecting at right angles within the body, let x, y, z, denote the co-ordinates that determine the position of dm, and a, b, c, those that determine the attracted point: then, if we put

$$r = \sqrt{a^2 + b^2 + c^2}$$
$$f = \sqrt{(a-x)^2 + (b-y)^2 + (c-z)^2};$$

r will be the distance of the attracted point from the origin of the co-ordinates, and f that of dm from the attracted point.

Now let $\mathrm{V}(r) = \int \frac{dm}{f}$,

the sign of integration, extending to all the molecules of the body: and $\mathrm{V}(r)$ will be the function alluded to, and which we have to consider.

It need not be mentioned that $\mathrm{V}(r)$ is not a function of r, but of the three co-ordinates a, b, c; or it is an abridged symbol denoting a function of *r*, and the angles which that line makes with the axes of the co-ordinates.

The distinguishing property of the expression $V(r)$ is this: if we take its fluxions with respect to the variable quantities *a*, *b*, *c*, the differential coefficients $-\left(\frac{d.V(r)}{da}\right)$, $-\left(\frac{d.V(r)}{db}\right)$, $-\left(\frac{d.V(r)}{dc}\right)$, will be respectively equal to the accumulated attractions of all the molecules of the body on the attracted point in the directions of *a*, *b*, *c*, and tending to shorten these lines.

Suppose now another body similar to the first in its lineal dimensions, and likewise having the parts similarly situated of the same density. If, therefore, this second body be divided into the same number of similar molecules as the first body; every two molecules, dm and dm', situated alike, will be of equal density, and their volumes will be proportional to the volumes of the two whole bodies. Suppose, also, that z', y', z', are three rectangular co-ordinates of the molecule dm', drawn to planes situated in the second body, similarly to the like planes in the first; and farther, let a', b', c', be the co-ordinates of an attracted point, placed in the same relative situation in the second body, as the former attracted point in the first; then,

$$r' = \sqrt{a'^2 + b'^2 + c'^2}$$

$$f' = \sqrt{(a' - x')^2 + (b' - y')^2 + (c' - z')^2}$$

$$V'(r') = \int \frac{dm'}{f'}.$$

It is manifest from what has been said, that *r* and *r'*, *f* and *f'*, are homologous lines of the two bodies; and hence,

$$\frac{r}{f} = \frac{r'}{f'}; \quad \frac{dm}{r^3} = \frac{dm'}{r'^3}:$$

consequently,

$$\frac{1}{r^2}\cdot\frac{dm}{f} = \frac{1}{r'^2}\cdot\frac{dm'}{f'}:$$

and as this is universally true of all the molecules, we have,

$$\frac{V(r)}{r^2} = \frac{V'(r')}{r'^2}.$$

In this form the expression is inconvenient, because both the quantities become infinite when we suppose that the attracted point is placed in the origin of the co-ordinates. But the inconvenience is easily removed by substituting, for r and r', any other two homologous lines. Let r and r', produced if necessary, meet the surfaces; and let R and R' denote the parts within the two bodies, intercepted between the surface and the origin of the co-ordinates: then it is manifest that $\frac{R}{r} = \frac{R'}{r'}$; and we shall therefore have

$$\frac{V(r)}{R^2} = \frac{V'(r')}{R'^2}.$$

If, therefore, we suppose a series of such bodies as we have been describing, which increase in magnitude from zero to infinity, while they constantly preserve the same proportions in their lineal dimensions, and the same densities of the parts similarly situated; the quantity $\frac{V(r)}{R^2}$, will have constantly the same value in all the bodies, supposing that the attracted points are alike placed in them all. It is manifest, therefore, that $\frac{V(r)}{R^2}$ will depend only on what is common to all the bodies in the series; or it will be a function of the quantities that remain unchanged in passing from one of the bodies to another. But as these quantities are not the same in all positions of the attracted point, it will be proper to distinguish several cases.

First, let the attracted point be in the surface, in which case $r = \mathrm{R}$: then, $\frac{a}{\mathrm{R}}, \frac{b}{\mathrm{R}}, \frac{c}{\mathrm{R}}$ are the only quantities that constantly retain the same values in all the bodies. These quantities remain unchanged, because the line r, or R, always makes the same angles with the axes of the co-ordinates. We therefore have,

$$\frac{\mathrm{V}(\mathrm{R})}{\mathrm{R}^2} = \mathrm{F}.\left\{\frac{a}{\mathrm{R}}, \frac{b}{\mathrm{R}}, \frac{c}{\mathrm{R}}\right\},$$

the letter F being the mark of a function. Hence,

$$\mathrm{V}(\mathrm{R}) = \mathrm{R}^2 \times \mathrm{F}.\left\{\frac{a}{\mathrm{R}}, \frac{b}{\mathrm{R}}, \frac{c}{\mathrm{R}}\right\}.$$

Again, let us put,

$$a = r.\mu;\ b = r\sqrt{1-\mu^2}.\,\mathrm{Cos.}\,\varpi;\ c = \gamma\sqrt{1-\mu^2}.\,\mathrm{Sin.}\,\varpi;$$

and μ will be the cosine of the angle which the line r, or R, makes with the axis of a; and ϖ the angle which the projection of the same line upon the plane of b and c makes with the axis of b: then, when $r = \mathrm{R}$, we have,

$$\mathrm{V}(\mathrm{R}) = \mathrm{R}^2 \times \mathrm{F}.\left\{\mu, \sqrt{1-\mu^2}.\,\mathrm{Cos.}\,\varpi, \sqrt{1-\mu^2}.\,\mathrm{Sin.}\,\mu\right\}.$$

Secondly, suppose that the attracted point is placed within each of the bodies; then the quantities common to them all are these, viz. $\frac{r}{\mathrm{R}}, \frac{a}{r}, \frac{b}{r}, \frac{c}{r}$. Hence,

$$\frac{\mathrm{V}(r)}{\mathrm{R}^2} = \mathrm{F}.\left\{\frac{r}{\mathrm{R}}, \frac{a}{r}, \frac{b}{r}, \frac{c}{r}\right\}.$$

Consequently,

$$\mathrm{V}(r) = \mathrm{R}^2 \times \mathrm{F}.\left\{\frac{r}{\mathrm{R}}, \frac{a}{r}, \frac{b}{r}, \frac{c}{r}\right\},$$

$$\mathrm{V}(r) = \mathrm{R}^2 \times \mathrm{F}.\left\{\frac{r}{\mathrm{R}}, \mu, \sqrt{1-\mu^2}.\,\mathrm{Cos.}\,\varpi, \sqrt{1-\mu^2}.\,\mathrm{Sin.}\,\varpi\right\}.$$

In order to have a more exact notion of this function, we may suppose it to be expanded in a series of the powers of the fraction $\frac{r}{\mathrm{R}}$: then,

$$V(r) = R^2 \times \left\{ P^{\circ} + P^{(1)} \cdot \frac{r}{R} + P^{(2)} \cdot \frac{r^2}{R^2} + \&c. \right\},$$

the coefficients P°, $P^{(1)}$, $P^{(2)}$, &c. being all functions of μ, $\sqrt{1-\mu^2}$. Cos. ϖ, $\sqrt{1-\mu^2}$. Sin. ϖ. When the attracted point coincides with the origin of the co-ordinates, the value of $\frac{V(0)}{R^2}$ is equal to $P^{(0)}$; and when the same point is in the surface, then $\frac{r}{R} = 1$, and the value of $\frac{V(R)}{R^2}$ is equal to

$$P^{(0)} + P^{(1)} + P^{(2)} + \&c.$$

Finally, let the attracted point be without the surface; then the quantities common to all the bodies are these, viz. $\frac{R}{r}$, $\frac{a}{r}$, $\frac{b}{r}$, $\frac{c}{r}$: hence

$$\frac{V(r)}{R^2} = F \,.\, \left\{ \frac{R}{r}, \frac{a}{r}, \frac{b}{r}, \frac{c}{r} \right\}.$$

Consequently,

$$V(r) = R^2 \times F \,.\, \left\{ \frac{R}{r}, \frac{a}{r}, \frac{b}{r}, \frac{c}{r} \right\};$$

$$V(r) = R^2 \times F \,.\, \left\{ \frac{R}{r}, \mu, \sqrt{1-\mu^2}\ \text{Cos.}\ \varpi, \sqrt{1-\mu^2}\,.\ \text{Sin.}\ \varpi \right\}.$$

In this case, $\frac{V(r)}{R^2}$ decreases as r increases, and finally vanishes when r is infinite. The expansion must therefore have this form, viz.

$$V(r) = R^2 \times \left\{ Q^{(1)} \cdot \frac{R}{r} + Q^{(2)} \cdot \frac{R^2}{r^2} + Q^{(3)} \cdot \frac{R^3}{r^3} + \&c. \right\},$$

$Q^{(1)}$, $Q^{(2)}$, $Q^{(3)}$, &c. being functions of μ, $\sqrt{1-\mu^2}$. Cos. ϖ, $\sqrt{1-\mu^2}$. Sin. ϖ. When the attracted point is in the surface, $\frac{R}{r^2} = 1$, and the value of $\frac{V(R)}{R^2}$ is equal to

$$Q^{(1)} + Q^{(2)} + Q^{(3)} + \&c.$$

The preceding reasoning is quite general, and will apply to any material body, whatever be its form. The body may consist of parts not connected by any mathematical law; or, which is the same thing, it is not necessary that the equation of its surface be subject to the law of continuity.

2. The co-ordinates of the molecule dm being x, y, z, let R′ denote its distance from the origin of the co-ordinates; and put,

$$x = R'.\mu'; \; y = R\sqrt{1-\mu'^2}.\text{Cos.}\,\varpi'; \; z = R'\sqrt{1-\mu'^2}.\text{Sin.}\,\varpi';$$

then, since we likewise have,

$$a = r.\mu; \; b = r.\sqrt{1-\mu^2}.\text{Cos.}\,\varpi; \; c = r\sqrt{1-\mu^2}.\text{Sin.}\,\varpi;$$

and,

$$f = \sqrt{(a-x)^2 + (b-y)^2 + (c-z)^2};$$

we shall get,

$$\gamma = \mu\mu' + \sqrt{1-\mu^2}.\sqrt{1-\mu'^2}.\text{Cos.}\,(\varpi - \varpi'),$$

$$f = \sqrt{r^2 - 2rR'.\gamma + R'^2}.$$

It now becomes necessary to expand $\frac{1}{f}$ in a series of the powers of $\frac{r}{R}$, or of $\frac{R}{r}$. Much has already been written on this expansion. The coefficients have been exhibited in various forms, and many remarkable properties which they possess have been very diligently explored. It would not, therefore, be necessary to add any thing upon this subject, unless it be possible to give to the same quantities a new and more simple form of expression, useful in the present investigation.

If we suppose,

$$\frac{1}{f} = \frac{1}{r} \cdot \left\{ 1 + C^{(1)}.\frac{R'}{r} + C^{(2)}.\frac{R'^2}{r} \ldots\ldots + C^{(i)}\frac{R'^i}{r^i} + \&c. \right\};$$

the following differential equation has already been proved in the Philosophical Transactions for 1812, viz.

$$i(i+1).C^{(i)} + \frac{d.\left\{(1-\gamma^2)\frac{dC^{(i)}}{d\gamma}\right\}}{d\gamma} = 0:$$

and from this equation the value of $C^{(i)}$ is deduced, viz.

$$C^{(i)} = \frac{1.3.5\ldots 2i-1}{1.2.3\ldots\ldots i} \times \left\{\gamma^i - \frac{i(i-1)}{2.2i-1}.\gamma^{i-2} + \text{\&c.}\right\}.$$

In the same place another more general differential equation is found, of which the former is only a particular case, viz.

$$(i-n).(i+n+1).(1-\gamma^2)^n\frac{d^nC^{(i)}}{d\gamma^n} + \frac{d.\left\{(1-\gamma^2)^{n+1}\frac{d^{n+1}C^{(i)}}{d\gamma^{n+1}}\right\}}{d\gamma} = 0.$$

For the sake of abridging I shall now put

$$\varphi^{(n)} = (1-\gamma^2)^n.\frac{d^nC^{(i)}}{d\gamma^n};$$

and, consequently,

$$\varphi^{(0)} = (1-\gamma^2)^0\frac{d^0C^{(i)}}{d\gamma^0} = C^{(i)}:$$

then, if in the foregoing formula, we make n successively equal to 0, 1, 2, 3 : . . i, we shall get this series of equations, viz.

$$\varphi^{(0)} + \frac{d.\varphi^{(1)}}{i(i+1.d\gamma} = 0$$

$$\varphi^{(1)} + \frac{d.\varphi^{(2)}}{i-1.i+2.d\gamma} = 0$$

$$\varphi^{(2)} + \frac{d.\varphi^{(3)}}{i-2.i+3.d\gamma} = 0$$

$$\vdots$$

$$\varphi^{(i-1)} + \frac{d.\varphi^{(i)}}{1.2i.d\gamma} = 0.$$

Now, by combining these equations, after having taken the fluxions of each a proper number of times, all the intermediate quantities between $\varphi^{(0)}$ and $\varphi^{(i)}$ may be made to disappear; and we shall finally obtain,

$$\varphi^{(0)} - \frac{(-1)^i}{1.2.3\ldots 2i} \times \frac{d^i\varphi^{(i)}}{d\gamma^i} = 0:$$

and by restoring the expressions that $\varphi^{(0)}$ and $\varphi^{(i)}$ stand for,

$$C^{(i)} = \frac{(-1)^i}{1.2.3\ldots 2i} \times \frac{d.^i\left\{(1-\gamma^2)^i \frac{d^i C^{(i)}}{d\gamma^i}\right\}}{d\gamma^i}.$$

But, from the series equal to $C^{(i)}$, we get

$$\frac{d^i C^{(i)}}{d\gamma^i} = 1.3.5\ldots(2i-1).$$

Wherefore,

$$C^{(i)} = \frac{(-1)^i}{2.4.6\ldots 2i} \times \frac{d^i(1-\gamma^2)^i}{d\gamma^i}.$$

From this very simple expression, the most remarkable properties of the coefficients of the expansion of $\frac{1}{f}$, are very readily deduced.

3. We may suppose that the indefinitely small molecule dm is a parallelopiped, of which the height is equal to dR'; and the length to $\frac{R'd\mu}{\sqrt{1-\mu'^2}}$, the small line described by the motion of R' perpendicular to the plane of y, z; and the breadth to $R'd\varpi'\sqrt{1-\mu'^2}$, the small line described by the motion of R' parallel to the same plane. The volume of the molecule is therefore equal to $dR' \times \frac{R'd\mu'}{\sqrt{1-\mu'^2}} \times R'd\varpi'\sqrt{1-\mu'^2}$; and, if ϱ be put for its density, we shall have

$$dm = \varrho R'^2 dR' d\mu' d\varpi'.$$

Again, we have,

$$f = \sqrt{r^2 - 2rR'\gamma + R'^2},$$

$$V(r) = \int \frac{dm}{f};$$

and, by taking the fluxions with regard to r,

$$-\left(\frac{d.V(r)}{dr}\right) r = \int \frac{dm\,(r^2 - rR'\gamma)}{f^3}.$$

But,

$$r^2 - rR'\gamma = f^2 + rR'\gamma - R'^2;$$

wherefore,

$$-\left(\frac{d.V(r)}{dr}\right) r = \int \frac{dm}{f} + \int \frac{dm\,(rR'\gamma - R'^2)}{f^3};$$

and, by adding the equivalent quantities, $2\,V(r)$ and $2\int \frac{dm}{f}$, we get

$$2\,V(r) - \left(\frac{d.V(r)}{dr}\right) r = 3\int \frac{dm}{f} + \int \frac{dm\,(rR'\gamma - R'^2)}{f^3};$$

and by substituting the value of dm,

$$2\,V(r) - \frac{d.V(r)}{dr}\, r = \iint d\mu'\, d\varpi' . \int \varrho . \left\{ \frac{3R'^2\, dR'}{f} + \frac{R'^3\,(r\gamma - R')\, dR'}{f^3} \right\}$$

or, which is the same thing,

$$-\left(\frac{d.\frac{V(r)}{r^2}}{dr}\right) r^3 = \iint d\mu'\, d\varpi' . \int \left(\frac{d.\frac{R'^3}{f}}{dR'}\right) \varrho\, dR'.$$

In the present paper, we confine our attention to a homogeneous body, or fluid mass; and, ϱ being constant, we may suppose it equal to unit; then, having integrated the last equation with regard to the variable R', we shall get,

$$-\left(\frac{d.\frac{V(r)}{r^2}}{dr}\right) r^3 = \iint \frac{R'^3\, d\mu'\, d\varpi'}{f}.$$

In this formula, R' is the line drawn from the origin of the co-ordinates to the surface; and the integration with respect to μ', is to extend from $\mu' = 1$ to $\mu' = -1$; and that with respect to ϖ', from $\varpi' = o$ to $\varpi' = 2\pi$, or the whole circumference.

The preceding formula is true, whether the attracted point be without or within the body. There is however a distinction between the two cases. If we multiply by $-\frac{dr}{r^3}$, and then integrate, we shall get

$$\frac{V(r)}{r^2} = \int -\frac{dr}{r^3} \iint \frac{R'^3\, d\mu'\, d\varpi'}{\sqrt{r^2 - 2rR'\gamma + R'^3}},$$

no constant quantity being necessary when the attracted point is without the body, because both the quantities vanish when r is infinitely great. But when the attracted point is within the body, it is necessary to add a constant quantity, because $\frac{dd.V(r)}{dr^2}$, is not evanescent when $r = o$: in this case, therefore, we have

$$\frac{V(r)}{r^2} = \int -\frac{dr}{r^3} \iint \frac{R'^3\, d\mu'\, d\varpi'}{\sqrt{r^2 - 2rR'\gamma + R'^2}} + K;$$

and

$$V(r) = r^2 \int -\frac{dr}{r^3} \iint \frac{R'^3\, d\mu'\, d\varpi'}{\sqrt{r^2 - 2rR'\gamma + R'^2}} + Kr^2,$$

K being a quantity independent of r.

It is necessary to find an expression of the value of K. For this purpose we have,

$$V(r) = \int \frac{dm}{f} = \iiint \frac{R'^2\, dR'\, d\mu'\, d\varpi'}{\sqrt{r^2 - 2rR'\gamma + R'^2}}.$$

Expand the denominator in a series of the ascending powers of r; then,

$$\begin{aligned} V(r) = & \iiint R'\, dR'\, d\mu'\, d\varpi' \\ & + r \iiint dR'.\, C^{(1)}\, d\mu'\, d\varpi' \\ & + r^2 \iiint \frac{dR'}{R'} \cdot C^{(2)}\, d\mu'\, d\varpi' \\ & + r^3 \iiint \frac{dR'}{R'^2} \cdot C^{(3)}\, d\mu'\, d\varpi' \\ & + \&c. \end{aligned}$$

The integrations with respect to dR' should be executed from $R' = o$, to the value of R' at the surface of the body; which cannot be done, because all the terms after the two first would be infinite. Conceive a sphere to be described about the origin of the co-ordinates with the radius r; then the whole value of the function $V(r)$ will be equal to its value with respect to the sphere added to its value with respect to the matter between the sphere and the surface of the body. The attracted point being in the surface of the sphere, the first part of the value of $V(r)$ will be equal to $\frac{4\pi}{3} \times r^2$; and the second part will be found by integrating the foregoing expression, so that all the integrals shall vanish when $R' = r$. Thus we get,

$$\begin{aligned} V(r) = \tfrac{4\pi}{3} r^2 &+ \iint \tfrac{1}{2}(R'^2 - r^2)\, d\mu'\, d\varpi' \\ &+ r \iint (R' - r) \,.\, C^{(1)}\, d\mu'\, d\varpi' \\ &+ r^2 \iint (\text{Log.}\, R' - \text{Log.}\, r) \,.\, C^{(2)}\, d\mu'\, d\varpi' \\ &+ \tfrac{r^3}{1} \iint \left(\tfrac{1}{r} - \tfrac{1}{R'}\right) . C^{(3)}\, d\mu'\, d\varpi' \\ &+ \tfrac{r^4}{2} \iint \left(\tfrac{1}{r^2} - \tfrac{1}{R'^2}\right) . C^{(4)}\, d\mu'\, d\varpi' \\ &+ \&c. \end{aligned}$$

Now the integral $-\frac{1}{2}\iint r^2\, d\mu'\, d\varpi'$, taken between the limits $\mu' = 1$, $\mu = -1$; and $\varpi' = o$, $\varpi' = 2\pi$; is equal to $-2\pi r^2$. All the other parts of the above expression that contain r, are evanescent; because we have generally,

$$\iint C^{(i)}\, d\mu'\, d\varpi' = o.$$

In order to prove this, it is to be observed that μ, μ' and γ are the cosines of the three sides of a spherical triangle, and $\varpi - \varpi'$ is the angle opposite to the side whose cosine is γ:

now if we put ψ for the angle opposite to the side whose cosine is μ', we may write $d\gamma\, d\psi$ in place of $d\mu'\, d\varpi'$, making γ and ψ vary between the same limits as μ' and ϖ'. This is allowable; not that we must conceive the two fluxions as continually equal to one another, but because the total sum, between the prescribed limits, is, in either case, equal to the whole surface of the sphere. If now we substitute the value of $C^{(i)}$ given in § 2, the foregoing expression will become,

$$\frac{(-1)^i}{2.4.6\ldots 2i} \times \iint \frac{d^i.(1-\gamma^2)^i}{d\gamma^i} \times d\gamma \times d\psi:$$

and the integral is,

$$\frac{(-1)^i}{2.4.6\ldots 2i} \times 2\pi \times \frac{d^{i-1}.(1-\gamma^2)^i}{d\gamma^{i-1}};$$

a quantity which, being divisible by $1-\gamma^2$, is evanescent at both the limits of γ.

Omitting what has been proved to be evanescent in $V(r)$, and collecting into one sum all the parts multiplied by r^2, and separating them from the rest, we get

$$\begin{aligned} V(r) = {} & \iint \frac{R'^2}{2}.\, d\mu'\, d\varpi' + r^2 \times \left\{-\frac{2\pi}{3} + \iint \log.R' \times C^{(2)} d\mu'\, d\varpi'\right\} \\ & + r \iint R' C^{(1)} d\mu'\, d\varpi' \\ & - \frac{r^3}{1} \iint \frac{C^{(3)} d\mu'\, d\varpi'}{R'} \\ & - \frac{r^4}{2} \iint \frac{C^{(4)} d\mu'\, d\varpi'}{R'^2} \\ & - \&c. \end{aligned}$$

which expression may be thus written in finite terms, viz.

$$V(r) = r^2 \int -\frac{dr}{r^3} \iint \frac{R'^3\, d\mu'\, d\varpi'}{f} + r^2. \left\{-\frac{2\pi}{3} + \iint \log.R' \times C^{(2)}\, d\mu'\, d\varpi'\right\}$$

as will be evident by expanding $\frac{1}{f}$, and performing the integrations with respect to dr. By comparing this expression of $V(r)$ with the foregoing one, we get,

$$K = -\frac{2\varpi}{3} + \iint \log. R' \times C^{(2)} d\mu' d\varpi'.$$

From this expression it follows that K has the same value in all similar homogeneous bodies. Suppose another body, similar and homogeneous to the first, and having the axes of the co-ordinates similarly placed: let R' and $R'_{,}$ be lines drawn to the surface from the origin of the co-ordinates, and making the same angles with the axes; then, K' denoting the like quantity in the second body as K in the first, we have

$$K = -\frac{2\varpi}{3} + \iint \log. R' \times C^{(2)} d\mu' d\varpi',$$

$$K' = -\frac{2\varpi}{3} + \iint \log. R'_{,} \times C^{(2)} d\mu' d\varpi';$$

wherefore,

$$K - K' = \iint \log. \frac{R'}{R'_{,}} \times C^{(2)} d\mu' d\varpi'.$$

But the two bodies being similar, and R' and $R'_{,}$ lines similarly drawn in them, it follows that $\frac{R'}{R'_{,}}$ will remain unchanged, when μ' and ϖ' vary. Consequently,

$$K - K' = \log. \frac{R'}{R'_{,}} \times \iint C^{(2)} d\mu' d\varpi' = 0.$$

4. Having now laid down the properties of attraction to which we shall have occasion to refer, we are next to consider the conditions necessary to the equilibrium of a fluid mass. These were first reduced to a uniform mode of calculation by CLAIRAUT, in his Theory of the Figure of the earth. They are investigated in all the great treatises of rational mechanics; in the *Mecanique Analytique* of LAGRANGE, the *Mecanique Celeste* of LAPLACE, and the *Mecanique* of POISSON.

The English reader will likewise find the same investigations in a work published in 1821, under the title of *Elementary Illustrations of the Celestial Mechanics*, which is a translation of the first book of the *Mecanique Celeste*, and, in addition to the text, contains much valuable matter. Referring to these works, we shall first merely enumerate the chief properties of the equilibrium of a fluid mass, for the sake of recalling them to the recollection of the reader; and then make such an application of the general principles as our present purpose requires.

A heterogeneous fluid body cannot be in equilibrio, unless the outer surface be every where of the same density; and farther, unless particles of the same density be arranged in distinct strata in the interior of the mass. The pressure upon all equal spaces of every stratum of uniform density, that is, the force acting perpendicularly to them, and pushing them inward, must be equal. Hence, these are called level strata, or *couches de niveau*, because the direction of the accelerating force, or of gravity, is every where perpendicular to them. It is easy to perceive that the densities must decrease in approaching the outer surface. For, in two contiguous strata of different densities, if we take two molecules equal in volume, and placed at the same point of the separating surface; the common gravity acting upon both will produce a greater pressure in the denser molecule. Wherefore, if the denser matter were nearer the outer surface, it would penetrate into the rarer matter below it; which is contrary to the perfect separation of the strata of different densities.

Supposing all these conditions to be fulfilled, it readily follows that the fluid body will be in equilibrio. For the equal pressure upon every part of the upper surface of a level stratum, being propagated through the interior fluid, will act with equal force in an opposite direction upon every part of the lower surface; and hence, every molecule of the stratum will be equally pressed in all opposite directions.

When the fluid mass is homogeneous, the distinction of the level strata arising from the difference of their densities is lost; but the possibility of dividing it into any number of strata separated by level surfaces, is still a necessary condition of the equilibrium.

The condition, that every level surface must be a continuous curve stretching through the whole fluid mass, imposes a limitation on the forces with which the equilibrium is possible. All these curve surfaces are defined by a common equation between three independent co-ordinates; and as this equation is to be found by integrating an expression containing the co-ordinates and their fluxions, the operation must be practicable, without supposing any relation between the three variable quantities. Hence, the forces acting on the particles of the fluid must be such, that the three differential coefficients shall fulfil what is called the criterion of integrability; otherwise the equilibrium will be impossible. In determining the equilibrium of the fluid placed on the surfaces of the planets, the nature of the forces brought into action is such, that the problem is always free from contradictory conditions.

To come now to the main object we have in view,* conceive that H K I is a body of homogeneous fluid in equilibrio by the action of all the forces that urge its particles. Let x, y, z, be three rectangular co-ordinates of a point K in the surface; and put X, Y, Z, for the accelerating forces that act upon a particle at K respectively in the directions of the co-ordinates, and tending to diminish these lines. Suppose that K varies its position a little in the fluid's surface; then the condition that the resultant of the forces parallel to the co-ordinates, shall be perpendicular to that surface, is expressed by this equation, viz.

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* Theorie de la Figure de la Terre par Clairaut. Premiere Partie, cap. V. § XXI.

$$X\,dx + Y\,dy + Z\,dz = 0.$$

In order that the equilibrium be possible, the expression just set down must be a complete differential; which subjects the forces X, Y, Z, to the criterion of integrability. This condition being fulfilled, the equation of the fluid's surface will be,

$$\int (X\,dx + Y\,dy + Z\,dz) = C,$$

C being an arbitrary constant quantity. If, for the sake of brevity, we represent the preceding integral by φ, we shall have,

$$\varphi = C$$

$$X = \frac{d\varphi}{dx},\ Y = \frac{d\varphi}{dy},\ Z = \frac{d\varphi}{dz}.$$

Again, let

$$p = \sqrt{X^2 + Y^2 + Z^2};$$

then p is the resultant of the forces X, Y, Z; and it acts on a particle placed at K, in a direction perpendicular to the fluid's surface, and tending inward. It is the gravity at that point.

Suppose now that a stratum of fluid is laid upon the surface HKI, the thickness at K being equal to the indefinitely small line KS. The new pressure at K will be proportional to the superincumbent matter multiplied by p, or by the gravity which urges a particle inward. But, as the density is constant, the quantity of matter pressing at K, will be proportional to the thickness KS. Wherefore, if δC represent the additional pressure at K, we shall have

$$\delta C = p \times KS.$$

Hence,

$$KS = \frac{\delta C}{p} = \frac{\delta C}{\sqrt{X^2 + Y^2 + Z^2}}.$$

If now we suppose that δC remains constant, and, by means of the formula just set down, determine the thickness at every point; it is evident that the stratum will press equably upon the surface of the fluid HKI, and consequently will not disturb the equilibrium by its pressure. It remains to determine the equation of the upper surface of the stratum. For this purpose we have,

$$\delta C = p \times KS = \frac{p^2}{p} \times KS = \left(X\frac{X}{p} + Y\frac{Y}{p} + Z\frac{Z}{p}\right) \times KS.$$

The co-ordinates of the point K being x, y, z, let those of the point s be $x + \delta x$, $y + \delta y$, $z + \delta z$: then KS being perpendicular to the surface HKI, it is easy to prove that,

$$\delta x = \frac{X}{\sqrt{X^2 + Y^2 + Z^2}} \times KS = \frac{X}{p} \times KS,$$

$$\delta y = \frac{Y}{p} \times KS$$

$$\delta z = \frac{Z}{p} \times KS;$$

wherefore, by substitution, we get

$$\delta C = X\,\delta x + Y\,\delta y + Z\,\delta z;$$

that is,

$$\delta C = \frac{d\varphi}{dx}\delta x + \frac{d\varphi}{\delta y}\delta y + \frac{d\varphi}{dz}\delta z.$$

Consequently,

$$\varphi + \frac{d\varphi}{dx}\delta x + \frac{d\varphi}{dy}\delta y + \frac{d\varphi}{dz}\delta z = C + \delta C.$$

Now this expression is derived from the equation,

$$\varphi = C,$$

on the one hand, by changing C into $C + \delta C$; and, on the other, by substituting, in the function φ, the co-ordinates $x + \delta x$, $y + \delta y$, $z + \delta z$ of the upper surface of the stratum, in place of x, y, z, the co-ordinates of the surface H K I. Thus it appears, that the equation of the new fluid body is derived from that of the first one, merely by varying the constant introduced in the integration.

Before proceeding farther, it is requisite to distinguish carefully two separate cases. The first is, when the particles of the fluid do not attract one another; and the second, when they are endowed with attractive powers. These are plainly two cases essentially different from one another: for, in the first, a stratum added induces no other change than an increase of pressure; but, in the second, besides the pressure a new force is introduced, arising from the attraction which the matter of the stratum exerts upon the fluid body to which it is added.

In the first case, when there are no new forces introduced by attraction, it is manifest from what has been said, that the fluid body of which the equation is,

$$\varphi = C + \delta C$$

is in equilibrio; because the stratum presses equally upon all parts of the surface H K I. If we suppose a second stratum to be laid upon the first, and compute its thickness by the gravity at the surface N O L, in the same manner that the thickness of the first was determined by means of the gravity at the surface H K I, we shall have another fluid body in equilibrio, of which the equation will be,

$$\varphi = C + \delta C + \delta' C$$

$\delta' C$ being equal to the pressure caused by the new stratum. And, in like manner, any number of strata may be added composing a fluid body in equilibrio.

But as strata have been added without disturbing the equilibrium, in like manner any number of strata below the surface HKI, may be successively taken away, so as to leave the remaining fluid in equilibrio. The original body HKI may be thus exhausted, or reduced to an infinitesimal quantity that may be neglected; and then the whole mass, both above and below the surface HKI, will consist of level strata separated by surfaces having a common equation, in which the constant quantity introduced in the integration alone varies in passing from one surface to another. We may therefore conclude that, when the particles of the fluid do not attract one another, the only conditions necessary for the equilibrium are, first, that the force resulting from X, Y, Z, be directed into the interior of the mass; and, secondly, that $X\,dx + Y\,dy + Z\,dz$ be an exact differential.

But this first case can have no application in the theory of the figure of the planets, the leading principle of which is, that every particle of matter attracts every other particle. We must therefore proceed to consider what new conditions are required in the second case, when the particles are possessed of attractive powers.

All the forces, whether attractive or not, that urge the particles of the fluid body HKI, are supposed to be included in the expressions X, Y, Z; and it has been shown that the gravity arising from these forces produces, by its action upon the stratum of which the thickness is KS, equal pressures upon every point of the surface HKI. The whole mass of fluid, NOL, will therefore be in equilibrio, if it be urged by no other forces. But the attraction of the stratum upon all the matter within it, is a new force brought into play, the efforts of which must be balanced, otherwise the equilibrium could not subsist. Now this new force is distinct from the pressure caused by the gravity, and can never be included in it. Two separate principles must therefore be employed to ensure the equilibrium of the fluid body HKI, when acted upon by the two independent forces. But a fluid body cannot be in equilibrio by the action of external forces upon it, except in one of these two ways: either there must be an equable pressure upon the outer surface; or, all the forces that act upon every separate particle must destroy one another. We are therefore necessarily led to suppose, that the added stratum must possess such a figure as to attract every particle in the inside with equal force in all opposite directions. By the help of this principle, and by no other means, the fluid body HKI, will still continue to be in equilibrio when subjected to the additional pressure, and to the new attractive force.

When more strata are added, they must separately possess the property of attracting every particle in the inside with equal force in opposite directions; by which supposition, we are brought at every step to the same circumstances, as in the case when there is no attraction between the particles. The whole fluid mass being ultimately divided into level strata, the property common to each must belong to the aggregate of any number of them.

On the whole, it is not sufficient for the equilibrium of a homogeneous fluid body, the particles of which attract one another, that the resultant of the forces X, Y, Z, be directed inward, and that $X\,dx + Y\,dy + Z\,dz$ be an exact differential: to these conditions it is necessary to add that, every particle placed within a stratum bounded by two level surfaces, be in equilibrio by the attraction of the stratum.

The conclusion we have arrived at does not coincide with the usual determination of the equilibrium of a fluid mass, in which no distinction is made between the two cases when the particles attract one another, and when they possess no such powers. The difference arises from this, that no notice is commonly taken of the attraction which the thin level stratum exerts upon the fluid body to which it is added. Every difficulty respecting this point will be removed, if it be impressed on the mind that the gravity at any level surface, and the pressure caused by it, are forces distinct from, and independent of, the attraction of the exterior matter. In estimating the pressure, the exterior fluid is unavoidably regarded merely as inert matter subjected to external force; and when there are active powers inherent in it, the effect of these must be separately investigated. It is said that nothing more is requisite to the equilibrium of a homogeneous fluid, than that the pressure be equable over all the outer surface. For, it is argued, since there is no distinction of density in the interior, it is always possible to trace curves that shall cut at right angles the resultants of all the forces urging the particles; which curves will therefore be level surfaces. But the defectiveness of this reasoning will appear if it be observed that, as every particle of the fluid is attracted by the whole mass, the curve surfaces traced in the manner described, will be entirely dependent upon the outer surface. If the uppermost stratum, or any number of the uppermost strata be taken away, a part of the attractive force acting upon every particle will be destroyed, and the curve surfaces will no longer be perpendicular to the resultants of the remaining forces urging the particles. Suppose that the strata are taken away successively; then, the figure necessary to the equilibrium of the remaining fluid will change as each stratum is abstracted; which is contrary to the just principles of the equilibrium of a fluid mass. The level surfaces of a homogeneous fluid mass in equilibrio, are determined without ambiguity by varying the arbitrary constant of the general equation. And as there is no doubt that the figure of the outer surface has no relation to any matter placed without it; so any level surface, which is defined by a perfectly similar equation, must be independent of all the exterior matter. Farther, the gravitation acting at any point of the outer surface is a function of the co-ordinates of that point, and has no dependence upon any exterior matter; and, the like force at any level surface being the same function of the co-ordinates of that surface, it must be equally independent of the exterior matter. And although it be admitted that every level surface must be perpendicular to the resultant of all the forces urging the particles, yet it does not follow that no modification of the forces is necessary to the equilibrium. In reality, the foregoing observations prove that, if we reason consistently from what is allowed in the usual determination of the equilibrium of a fluid mass, we shall be led to the same conclusion at which we have already arrived; namely, that the forces acting at any point in the interior, must be so modified by the figure of the fluid, as to render every level surface, and the gravity at every point of it, independent of the exterior matter.

We may cite as examples of the two different cases of the equilibrium of a homogeneous fluid, the hypothesis of Huyghens respecting the figure of the earth, which falls under the first case; and the Newtonian theory on the same subject, which belongs to the second. Huyghen's supposed an attractive force residing in the earth's centre, and acting with the same intensity at all distances. Therefore, in the case of a revolving mass, every particle is urged by a constant force directed to the centre, and by a centrifugal force proportional to the distance from the axis of rotation. As there is no attraction between particle and particle, a level stratum will act by pressure only upon the fluid below it; and the only condition requisite to the equilibrium, is an equable pressure over all the outer surface. But, according to Newton, every particle attracts every other particle; and a level stratum will act upon the fluid below it, both by the pressure of gravitation and by its own attractive force. In this theory, therefore, the adjustment of the equilibrium requires the joint application of both the principles of the second case.

The method of investigation followed in what goes before, is similar to a process of reasoning in Clairaut's theory of the figure of the earth; and it is certainly surprising that the difference of the two cases was not remarked by that acute geometer. Other authors have very generally adopted a more simple procedure introduced by Euler. It will be worth while to set before the reader very briefly the steps of EULER's investigation, for the purpose of pointing out the omission with which it is chargeable.

Suppose that the fluid mass in equilibrio is divided into indefinitely small rectangular parallelopipeds by planes parallel to those of the co-ordinates; and let x, y, z, be the co-ordinates of one angle of a parallelopiped which has dx, dy, dz, for its sides, and which we may conceive to be so placed, that $x + dx$, $y + dy$, $z + dz$, are the co-ordinates of the opposite angle. The forces that act upon the parallelopiped are; the pressure of the adjacent fluid upon its six faces; and the accelerating forces X, Y, Z, urging every particle in directions parallel to x, y, z, and tending to increase these lines. The pressure at any point of the fluid must depend upon the situation of that point, or it must be some function ϕ of the co-ordinates x, y, z: and, according to the principles of the differential calculus, ϕ will retain the same value over all the three faces of the parallelopiped that comprehend any one of the solid angles. Now, y and z remaining constant, if we substitute $x + dx$ in place of x, ϕ will be changed into $\phi + \frac{d\phi}{dx} . dx$; and the two quantities ϕ and $\phi + \frac{d\phi}{dx} dx$, will represent the intensities of pressure upon the opposite rectangles comprehended by dy and dz: the forces compressing the parallelopiped are therefore $\phi \times dy\,dz$, and $(\phi + \frac{d\phi}{dx} dx)$ $dy\,dz$; and the difference, or $\frac{d\phi}{dx} dx\,dy\,dz$ is the force causing the parallelopiped to move in a direction tending to diminish x. In like manner, the pressures on the other sides produce the forces $\frac{\phi}{dy} dx\,dy\,dz$ and $\frac{d\phi}{dz} dx\,dy\,dz$, causing the parallelopiped to move in directions tending to diminish y and z. Again, the accelerating forces X, Y, Z, acting on every parallelopiped produce the motive forces $X\,dx\,dy\,dz$, $Y dx dy dz$, $Z\,dx\,dy\,dz$, tending to increase the lines x, y, z. But the equilibrium of the parallelopiped requires the equality of the opposite forces: wherefore,

$$\frac{d\varphi}{dx} = X, \frac{d\varphi}{dy} = Y, \frac{d\varphi}{dz} = Z.$$

Hence, we get,

$$d\varphi = X\,dx + Y\,dy + Z\,dz.$$

Wherefore if we trace a stratum of the fluid so that φ shall every where have the same value, the figure of the stratum will be defined by the equation

$$X\,dx + Y\,dp + Z\,dz = o;$$

which likewise shows that the resultant of the accelerating forces is perpendicular to the stratum.

In what has been said, the equilibrium of every parallelopiped is established with respect to all the outward forces extrinsic to its own matter. If the question relate to no other forces, the whole fluid mass, and all the level strata of which it consists, will be in equilibrio, and the problem is solved. But when the particles of the fluid attract one another, there are forces not yet taken into account, inherent in every parallelopiped, by means of which it will act upon all the exterior matter, and the efforts of which must be balanced, otherwise the equilibrium could not subsist. Now, if we suppose, as before, that all the level strata are possessed of such a figure as to act upon particles in the inside with equal forces in opposite directions, it is evident, that every parallelopiped will be in equilibrio by its action upon all the matter on the outside of the stratum. With regard to the matter in the inside, a parallelopiped will act upon it effectively; but, the united attraction of all the parallelopipeds in the same stratum upon every interior particle being equal in opposite directions, it will not disturb the equilibrium of the fluid below the stratum. Therefore, when we take into account all the forces that act upon the parallelopipeds; both those urging them externally, and those inherent in their own matter; it is evident, that all the molecules in the same level stratum will be in equilibrio with respect to the matter above them, and that they will press equably upon the fluid body below them, by the action of the gravity alone. The fluid mass will therefore be in equilibrio with respect to all the forces in action. Thus, in every view of the problem, it appears that, when nothing essential is omitted, the particular conformation of the level strata which annihilates their action upon particles in the inside, is just as necessary to the equilibrium of the fluid mass, as the equality between the pressure and the effect of the accelerating forces.

There is another way of arriving at the same conclusion, which, in reality, first led to the suspicion of some defect lurking in the usual determination of the equilibrium of a fluid mass. This new view of the subject, which applies only to the law of attraction that takes place in nature, is contained in the two following propositions.

PROPOSITION I.

If a homogeneous fluid body revolving about an axis, be in equilibrio by the attraction of its particles in the inverse proportion of the square of the distance; any other mass of the same fluid having a similar figure, and revolving with the same rotatory velocity about an axis similarly placed, will likewise be in equilibrio, supposing that its particles attract one another by the same law.

Suppose that a homogeneous fluid body revolves about the axis A B, and is in equilibrio by the attraction of its particles and the centrifugal force; and let another mass of the same fluid, similar in its figure to the first body, revolve, in the same time, about the axis *a b*, similarly situated to A B: this latter body will also be in equilibrio.

Conceive that the body in equilibrio is divided into an indefinitely great number of thin level strata; and let the other body be divided into the same number of strata by surfaces similar, and similarly situated to the level surfaces of the first body. Take any point H in a level surface of the body in equilibrio; and in the corresponding surface of the other body, let the point *h* be similarly situated to H. Farther, suppose the two bodies are similarly divided into the same indefinitely great number of molecules, of which dm and dm' are any two situated alike, and therefore having their volumes and quantities of matter proportional to the volumes and quantities of matter of the two whole bodies: and let f and f' denote the respective distances of the points H and h from dm and dm', and r and r', their respective distances from the axes A B and *a b*.

The forces with which the molecules dm and dm' attract the points H and h (which must be considered as two equal particles of matter) are proportional to $\frac{dm}{f^2}$ and $\frac{dm'}{f'^2}$: and, in these fractions, the numerators being proportional to the cubes, and the denominators to the squares, of any two homologous lines of the respective bodies, the attractive forces will be simply proportional to any two such lines. The lines *f* and *f′*, in the directions of which the forces act, are likewise similarly inclined to the surfaces passing through the two points H and *h*. It follows, therefore, that the forces with which the similar molecules into which the two bodies are divided, attract the points H and *h*, are constantly in the same proportion to one another, and act in directions that make like angles with the surfaces passing through the same points. Farther, since the velocity of rotation is the same in the two bodies, the centrifugal forces urging the points H and *h* will be proportional to the respective distances from the axes A B and *a b*; that is, to *r* and *r′*, or to any homologous lines of the respective bodies; and the same forces, having their directions in the prolongations of *r* and *r′*, make like angles with the surfaces passing through H and *h*. Wherefore all the accelerating forces urging the points H and *h*, are respectively in the same proportion to one another, and have like inclinations to the surfaces passing through the same points. Consequently, the resultants of the same forces will follow the like proportion, namely, that of any homologous lines; and they will likewise be similarly inclined to the two surfaces. But the resultant of the accelerating forces acting at H, is perpendicular to the level surface passing through that point; wherefore, the resultant of the accelerating forces acting at *h*, is likewise perpendicular to the surface in which that point is placed, and has to the other resultant the same proportion of any two homologous lines of the respective bodies. And thus, as in the body in equilibrio, the gravity, or the resultant of the accelerating forces is every where perpendicular to the level surfaces; so in the other body, the like force is every where perpendicular to the surfaces similarly situated.

Take K and k any other two points similarly situated in the same surfaces that contain H and h: and suppose that HM, KN, are the thicknesses of the level stratum, in the upper surface of which H and K are placed; and, in like manner, let hm, kn, be the thicknesses of the like stratum in the other body. Farther, put G, G′ for the resultants of the accelerating forces, or the gravitations, at H and K; and g, g' for the like forces at h, k. Because H and h are points similarly situated, the forces G, g are proportional to any homologous lines of the respective bodies. The same thing is true of the forces G′, g'. Wherefore,

$$G : G' :: g : g'.$$

But the line HM is homologous to hm, and KN, to kn: wherefore,

$$HM : KN :: hm : kn.$$

Consequently,

$$G \times HM : G' \times KN :: g \times hm : g' \times km.$$

But the proportion of $G \times HM$ to $G' \times KN$ is equal to that of the pressures of the stratum upon the fluid below it at the points H and K: for the quantities of matter in the stratum are proportional to the thicknesses HM and KN; and the pressures are proportional to the gravitations multiplied by the quantities of matter. In like manner $g \times hm$ and $g' \times kn$, are proportional to the pressures of the stratum upon the fluid below it at the points h and k. Wherefore the pressures at H and K are proportional to the pressures at h and k. And, in general, taking any points similarly placed in the two corresponding surfaces, the pressures of the stratum upon the fluid below it in one body are in the same proportion to one another, as the pressures of the stratum upon the fluid below it in the other body. But in the body in equilibrio, the pressures at all points are equal; wherefore, in the other body, a stratum likewise presses equably upon the fluid below it. And what is true of each individual stratum, must be true of the accumulated pressure of any number of superincumbent strata.

Thus, in the two bodies, every thing is similar. The forces which urge the particles of one, are, in the case of the other, all increased, or all diminished, in the same proportion, while they act in like directions. If, in the one, the gravity be every where perpendicular to the level surfaces; the like force is perpendicular to the surfaces similarly traced in the other: and if, in the first, all the level strata press equably upon the fluid below them; the same thing is true of the strata into which the second is divided. Wherefore, the equilibrium of one body is a necessary consequence of the equilibrium of the other.

PROPOSITION II.

If a homogeneous fluid mass revolve about an axis, and be in equilibrio by the attraction of its particles in the inverse proportion of the square of the distance; all the level surfaces will be similar to the outer one: and any stratum of the fluid contained between two level surfaces will attract particles in the inside with equal force in opposite directions.

Suppose that the homogeneous fluid body R S T, revolving about the axis A B, is in equilibrio by the centrifugal force, and the attraction of its particles in the inverse proportion of the square of the distance. The axis of rotation A B, will pass through G, the centre of gravity of the fluid mass. In the interior of the revolving body, trace, round the point G, any surface HIK, similar and similarly situated to the outer surface. Then the whole fluid body RST, and the part of it bounded by the surface H I K, are similar to one another in their figure; and they revolve about the common axis AB, which cuts them both similarly: wherefore, because the first body is in equilibrio, the latter body will also be in equilibrio, supposing that it revolves by itself, the exterior matter being taken away, or annihilated.*

And, because the body HI K is in equilibrio, when it revolves by itself, the resultant of the forces acting at its surface (namely, the attraction of its own particles and the centrifugal force) will, at every point, be perpendicular to that surface.

Suppose now that all the fluid exterior to the surface HIK is divided into very thin strata by the surfaces O P Q, LMN, similar and similarly situated to the outer surface RST. Then, understanding by the gravitation at any of the surfaces OPQ, LMN, &c. the resultant of the centrifugal force and the attraction of the fluid matter within that surface, it has been proved that these gravitations are perpendicular to the respective surfaces. Wherefore the uppermost stratum will be pressed perpendicularly upon the surface OPQ by the gravitation at that surface. For the same reason the next stratum will be pressed perpendicularly upon the surface LMN. And, in like manner, the successive strata will be pressed perpendicularly, each upon the surface on which

* Proposition I.

it lies, by the respective gravitations. If we conceive a curve line GHLOR, extending from the common centre G so as to cut all the similar surfaces at right angles; that curve line will mark the directions both of the gravitation and the pressure of the fluid in the interior of the body RST. Wherefore the several surfaces OPQ, LMN, &c. are no other than the level surfaces of the body RST in equilibrio; and each of these surfaces will be pressed by the superincumbent fluid with the same intensity over its whole extent.

But at the same time that the uppermost stratum presses upon the fluid below it, by the gravitation at the surface OPQ, it likewise attracts every particle of matter within the same surface. And, in like manner, every successive stratum both presses on the surface on which it lies by the gravitation at that surface, and attracts all the particles within it. Wherefore the body HIK is not only pressed by the superincumbent fluid, but every particle of it, is likewise attracted by all the exterior matter. These forces are independent on one another. Although the body HIK be in equilibrio with respect to the pressure it sustains, it does not follow that it will likewise be in equilibrio with respect to the attraction which the exterior matter exerts upon it. In order that this latter equilibrium take place, it is necessary that every stratum of the exterior matter be possessed of such a figure as to attract all particles in the inside with equal force in opposite directions.

We have now proved that, if the fluid mass RST be in equilibrio, the interior body HIK will likewise be in equilibrio when it revolves by itself, the exterior matter being taken away, or annihilated; which cannot be the case, unless the same body H I K be in equilibrio with respect to the pressure and attraction which the exterior matter exerts upon it. It has likewise been proved that all the surfaces O P Q, L M N, &c. similar to the outer surface R S T, are level surfaces; and this ensures the equilibrium of the interior body H I K with respect to the pressure it sustains. Its equilibrium with respect to the attraction of the exterior matter, requires farther, that all the strata between the surfaces R S T, O P Q, L M N, &c. attract every particle within them equally in opposite directions. We are therefore to conclude that the homogeneous fluid body R S T, which revolves about the axis A B, and the particles of which attract one another in the inverse proportion of the square of the distance, cannot be in equilibrio, unless both these conditions be fulfilled; 1st. The level surfaces must be all similar to one another; 2dly. Every stratum contained between two level surfaces must attract particles in the inside with equal force in opposite directions.

In the Proposition that has just been proved, the similarity of the level surfaces is an accidental property connected with the supposed law of attraction. In the general hypothesis of an attractive power between the particles, the conditions of equilibrium are no more than these: 1st. The resultant of the accelerating forces acting at every point of the outer surface must be directed at right angles towards that surface: 2dly. All the level strata must possess such a figure as to attract particles in the inside with equal force in opposite directions. It may not be altogether superfluous to prove, by a synthetic demonstration, that these conditions are sufficient for the equilibrium. This is done in the following Proposition.

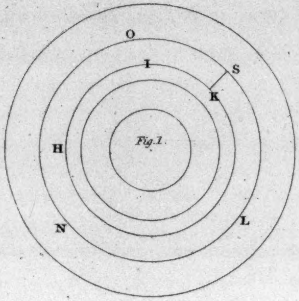

Fig. 1.

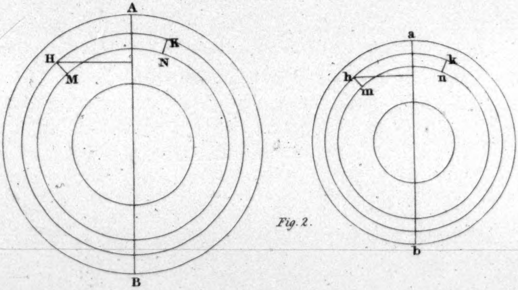

Fig. 2.

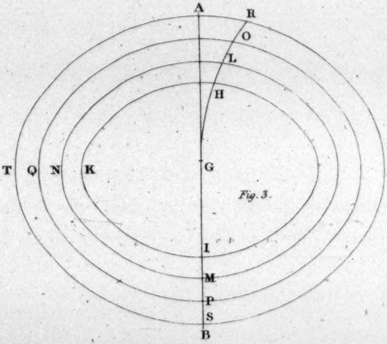

Fig. 3.

Jª Basire sc.

PROPOSITION III.

If a homogeneous fluid mass fulfil the two above-mentioned conditions, it is in equilibrio.

Let the equation of the outer surface of the fluid mass, be

$$\varphi = \mathrm{C};$$

φ representing a function of three rectangular co-ordinates, x, y, z. Then the accelerating forces parallel to the axes of x, y, z, will be respectively equal to $\frac{d\varphi}{dx}, \frac{d\varphi}{dy}, \frac{d\varphi}{dz}$; and the condition that the resultant of these forces is perpendicular to the surface of the fluid will be expressed by the differential equation,

$$\frac{d\varphi}{dx} dx + \frac{d\varphi}{dy} dy + \frac{d\varphi}{dz} dz = o.$$

We shall suppose that the whole fluid mass is divided into thin strata by level surfaces, which are determined by making the constant quantity C vary by insensible degrees in the equation of the outer surface. Farther, let the thickness of the uppermost stratum be denoted by the line k, drawn perpendicular to the outer surface from a point of which the co-ordinates are x, y, z; and let $x - \delta x$, $y - \delta y$, $z - \delta z$, be the co-ordinates of the other extremity of k in the under surface of the stratum. The equation of this surface will be found by substituting $x - \delta x$, $y - \delta y$, $z - \delta z$, in place of x, y, z, in the function φ, and by changing C into $\mathrm{C} - \delta \mathrm{C}$; it will therefore be,

$$\varphi - \frac{d\varphi}{dx} \delta x - \frac{d\varphi}{dy} \delta y - \frac{d\varphi}{dz} \delta z = \mathrm{C} - \delta \mathrm{C}:$$

and by subtracting this from the equation of the outer surface, we get

$$\frac{d\varphi}{dx}\delta x + \frac{d\varphi}{dy}\delta y + \frac{d\varphi}{dz}\delta z = \delta C.$$

Again; if we put,

$$p = \sqrt{\left(\frac{d\varphi}{dx}\right)^2 + \left(\frac{d\varphi}{dy}\right)^2 + \left(\frac{d\varphi}{dz}\right)^2};$$

then, because the line k is perpendicular to the outer surface, it follows, from the known properties of curve surfaces, that the cosines of the angles which k makes with the co-ordinates, are respectively equal to,

$$\frac{1}{p} \times \frac{d\varphi}{dx};\ \frac{1}{p} \times \frac{d\varphi}{dy};\ \frac{1}{p} \times \frac{d\varphi}{dz}:$$

and hence,

$$\delta x = \frac{k}{p} \times \frac{d\varphi}{dx};\ \delta y = \frac{k}{p} \times \frac{d\varphi}{dy};\ \delta z = \frac{k}{p} \times \frac{d\varphi}{dz}:$$

and by substituting these values in the preceding formula, we obtain,

$$\frac{k}{p} \times \left\{\left(\frac{d\varphi}{dx}\right)^2 + \left(\frac{d\varphi}{dy}\right)^2 + \left(\frac{d\varphi}{dz}\right)^2\right\} = \frac{k}{p} \times p^2 = k \times p = \delta C.$$

Now p is the resultant of the accelerating forces at the surface: and the line k, or the thickness, is proportional to the quantity of matter in the stratum at the same point; wherefore $k \times p$ is the pressure; and the formula,

$$k \times p = \delta C,$$

shows that the uppermost stratum presses upon the fluid below it equally at all points.

As the attraction of the whole fluid mass is one of the component forces of the gravitation p, the attraction of the stratum must enter as a part of the same force. But it is evidently only an infinitely small part of it; and consequently produces only an infinitely small part of the pressure $p \times k$. We may therefore consider the gravitation at the outer surface, and the pressure of the uppermost stratum upon the fluid below it, as both independent of the attractive force of the matter of the stratum. But the attraction of the same matter upon all the particles within the stratum is a force of the same order with the pressure $p \times k$, and comparable with it, and which must not be neglected. Thus it appears, that the uppermost stratum acts upon the fluid below it both by pressure and by attraction; and, as all the level strata are derived from one another by the same law, it follows, that every stratum in the interior likewise acts upon the fluid below it both by pressure and by attraction.

Now it has already been shown, that the pressure of the uppermost stratum is the same over all the surface of the fluid below it; and the same thing, it is manifest, is equally true of any level stratum. Wherefore, since the strata press equably upon one another, any fluid body in the interior, bounded by a level surface, will be in equilibrio with respect to the pressure it sustains from all the superincumbent strata. But, according to the second condition in the hypothesis of the proposition, the same body will also be in equilibrio with respect to the attraction of all the exterior strata. Thus, every interior fluid body bounded by a level surface, is in equilibrio with respect to all the forces which the exterior matter exerts upon it. And, as this is true independently of the dimensions of the interior body, we may suppose that it is ultimately reduced to a quantity infinitely small, which exerts no force, and is in equilibrio by the external forces acting upon it. Then the whole fluid mass will be resolved into level strata, that are in equilibrio with respect both to the pressures and to the attractive forces, which they exert upon one another. We are therefore to conclude, that the two conditions of the proposition are sufficient to ensure the equilibrium of the fluid mass, and that both are necessary to produce the effect.

5. Having established the physical properties of a homogeneous fluid mass in equilibrio, the investigation of its figure, which is now brought within the power of analysis, is not attended with much difficulty.

If a homogeneous body of fluid revolve about an axis, and be in equilibrio by the centrifugal force and the attraction of its particles, the axis of rotation will pass through the centre of gravity. This point is to be supposed at rest; since it is not the effect of any external forces that we have to consider, but merely the mutual action of the particles upon one another. If one of the planes of the co-ordinates pass through the centre of gravity at right angles to the axis of rotation, the other two will intersect one another in the same axis. Let a, b, c, denote the co-ordinates of an attracted point (which must be considered as some small particle of the fluid containing a given quantity of matter) placed any where in the mass, a being parallel to the axis of rotation; and put $r = \sqrt{a^2 + b^2 + c^2}$. Suppose also that $V(r)$ denotes the sum of all the molecules of the body divided by their respective distances from the attracted point: then $\frac{d.V(r)}{da}$, $\frac{d.V(r)}{db}$, $\frac{d.V(r)}{dc}$, will be the accumulated attractive forces exerted upon the attracted point by the whole mass, in the directions of a, b, c, and tending to increase these lines. Again; let ω be the centrifugal force at the distance from the axis of rotation equal to unit; then, the distance of the attracted point from the same axis being equal to $\sqrt{b^2 + c^2}$, the centrifugal force urging it from the axis, will be $\omega \times \sqrt{b^2 + c^2}$; and the effect of the same force to lengthen b and c, will be equal to $\omega \times \sqrt{b^2 + c^2} \times \frac{b}{\sqrt{b^2 + c^2}}$ and $\omega \times \sqrt{b^2 + c^2} \times \frac{c}{\sqrt{b^2 + c^2}}$, or to $\omega\, b$ and $\omega\, c$. Hence, the forces acting upon the attracted point, and tending to increase a, b, c, are respectively,

$$\frac{d\,.\,\mathrm{V}(r)}{d\,a},$$

$$\frac{d\,.\,\mathrm{V}(r)}{d\,b} + \omega\,.\,b,$$

$$\frac{d\,.\,\mathrm{V}(r)}{d\,b} + \omega\,.\,c.$$

Now, the resultant of these forces must be perpendicular to the level surface of which a, b, c are the co-ordinates; which condition is expressed by this differential equation, viz.

$$\frac{d\,.\,\mathrm{V}(r)}{d\,a}\,d\,a + \frac{d\,.\,\mathrm{V}(r)}{d\,b}\,d\,b + \frac{d\,.\,\mathrm{V}(r)}{d\,c}\,d\,c + \omega\,(b\,d\,b + c\,d\,c) = 0:$$

and the integral of this, viz.

$$\mathrm{V}(r) + \frac{\omega}{2}(b^2 + c^2) = \mathrm{C},$$

is the general equation of all the level surfaces. Let μ denote the cosine of the angle which the line r makes with the axis of rotation; and the foregoing equation will become,

$$\mathrm{V}(r) + \frac{\omega}{2}\,.\,r^2\,(1 - \mu^2) = \mathrm{C}.$$

And if we put R for the radius of the outer surface of the fluid body, we shall have, for the equation of that surface,

$$\mathrm{V}(\mathrm{R}) + \mathrm{R}^2 \times \frac{\omega}{2}(1 - \mu^2) = \mathrm{C}, \ldots \text{(A)}$$

which is one condition of the equilibrium of the fluid mass.

The equation just found is an essential condition of the equilibrium, although it is not the only one. As it merely expresses that the resultant of all the accelerating forces is perpendicular to the fluid's surface, it is not confined to a homogeneous body, nor to one entirely fluid, but is true in every case when a fluid in equilibrio covers, either entirely or partially, the surface of any body, however variable in structure or density. Now, from the equation, we get,

$$\frac{1}{R^2} = \frac{\frac{V(R)}{R^2} + \frac{\omega}{2}(1-\mu^2)}{C};$$

and, as it has been proved in §. 1, that $\frac{V(R)}{R^2}$ is always a function of three rectangular co-ordinates of a point in the surface of a sphere, it follows that $\frac{1}{R^2}$, and consequently R, must be like functions. This inference, being founded on considerations of the most general nature, cannot but include every case of a fluid in equilibrio, placed upon the surface of a revolving body.

Again, suppose that $R_,$ is the radius of any level surface which contains the attracted point within it; and let $V_,(r)$ denote the sum of all the molecules of the fluid within the level surface, divided by their respective distances from the attracted point: then,

$$V(r) - V_,(r)$$

will be the sum of the molecules in the stratum of fluid contained between the outer surface and the level surface, divided by their respective distances from the attracted point; and the attractive forces of the stratum upon the attracted point in the directions of *a*, *b*, *c*, will be respectively,

$$\frac{d.\left\{V(r) - V_,(r)\right\}}{da},$$

$$\frac{d.\left\{V(r) - V_,(r)\right\}}{db},$$

$$\frac{d.\left\{V(r) - V_,(r)\right\}}{dc}.$$

Now, in the case of a homogeneous mass of fluid in equilibrio, these forces must be evanescent; and that too, for every point within the stratum, which requires that

$$\mathrm{V}(r) - \mathrm{V}_{,}(r),$$

shall be a constant quantity independent of *a*, *b*, *c*. And this is the remaining condition necessary to the equilibrium.

Now, if we put,

$$\mathrm{Q} = r^2 \int -\frac{dr}{r^3} \iint \frac{\mathrm{R}'^3\, d\mu'\, d\varpi'}{\sqrt{r^2 - 2r\mathrm{R}'\gamma + \mathrm{R}'^2}}$$

$$\mathrm{Q}_{,} = r^2 \int -\frac{dr}{r^3} \iint \frac{\mathrm{R}'^3_{,}\, d\mu'\, d\varpi'}{\sqrt{r^2 - 2r\mathrm{R}'_{,}\gamma + \mathrm{R}'^2_{,}}};$$

we shall get,

$$\mathrm{V}(r) = \mathrm{Q} + \mathrm{K}r^2$$

$$\mathrm{V}_{,}(r) = \mathrm{Q}_{,} + \mathrm{K}_{,}r^2:*$$

consequently,

$$\mathrm{V}(r) - \mathrm{V}_{,}(r) = \mathrm{Q} - \mathrm{Q}_{,} + r^2(\mathrm{K} - \mathrm{K}_{,}).$$

A very little attention will show that Q and $\mathrm{Q}_{,}$, contain no terms multiplied by r^2. For if we expand Q into a series of the ascending powers of *r*, the term containing r^2, will be

$$r^2 \times \iint \mathrm{C}^{(2)}\, d\mu'\, d\varpi',$$

which, by the nature of the function $\mathrm{C}^{(2)}$, is equal to zero. Wherefore the foregoing expression cannot be independent of *a*, *b*, *c*, unless

$$\mathrm{K} - \mathrm{K}_{,} = 0;\ \text{and}\ \mathrm{K} = \mathrm{K}_{,}.$$

But it has already been shown that the equality of K to $\mathrm{K}_{,}$ requires that the radii R′ and $\mathrm{R}'_{,}$, which make the same angles with the axes of the co-ordinates, and consequently are in the same straight line, be constantly in the same proportion to one another:* and hence we obtain this property of a homogeneous fluid mass in equilibrio, namely, that all the level surfaces are similar to the outer one.

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* Section 3.

Again, since R and $R'_{,}$ are always in the same proportion to one another, $Q - Q_{,}$ will be independent of a, b, c, if we make r disappear in Q. Now, by expanding the expression of Q, and equating the co-efficients of the several powers of r to zero, we get,

$$Q = \int\int R'^2 \, d\mu' \, d\varpi$$

$$o = \int\int R' . C^{(1)} d\mu' \, d\varpi'$$

$$o = \int\int \frac{C^{(3)} d\mu' \, d\varpi'}{R'}$$

$$o = \int\int \frac{C^{(4)} d\mu' \, d\varpi'}{R'}$$

and generally,

$$o = \int\int \frac{C^{(i)} d\mu' \, d\varpi'}{R'^{i-2}}$$

In the first place, all these equations are satisfied if we suppose R' constant; that is, if the figure of the fluid be a sphere. But the supposition of a sphere is inconsistent with the equation (A), unless ω be evanescent. Wherefore, a homogeneous fluid body of a spherical figure cannot be in equilibrio by the attraction of its particles, unless it have no rotatory motion.

Again, it follows from what has been shown, that R' is a function of μ', $\sqrt{1 - \mu'^2}$. Sin ϖ, $\sqrt{1 - \mu'^2}$. Cos ϖ'; μ' being the cosine of an arc θ' reckoned from the pole of a great circle on the sphere, and ϖ' the angle between θ' and a given great circle passing through the same pole. Now if we suppose that R' is an even function, or that it contains only the squares and the combinations of the squares of $\mu'_{,}$, $\sqrt{1 - \mu'^2}$. Sin ϖ', $\sqrt{1 - \mu'^2}$. Cos ϖ'; the values of R', which are always positive, will be the same in quantity, at points diametrically opposite on the sphere, at which points μ', Sin. ϖ' and Cos. ϖ', are different only in their signs. And because,

$$\gamma = \mu\mu' + \sqrt{1-\mu^2} \, . \, \sqrt{1-\mu'^2} \, \text{Cos.} \, (\varpi - \varpi'),$$

it is obvious that γ will likewise have the same value and different signs at any two points diametrically opposite on the sphere; and the same property will belong to every function of γ that contains only the odd powers. Now we have

$$R' \, . \, C^{(1)} \, d\mu' \, d\varpi' = - R' \, . \, C^{(1)} \, d\theta' \, \text{Sin} \, \theta' \, d\varpi';$$

and, as θ' increases from o to π, and ϖ' from o to 2π, it is obvious that the fluxions will be the same in quantity, but will have different signs, at any two points diametrically opposite on the sphere; because the sign of $C^{(1)}$, which is an odd function of γ, alone changes. Wherefore the integral will decrease just as much in one hemisphere as it increases in the other; and being extended to the whole sphere, it will be equal to zero. In the same manner it is proved that

$$\iint \frac{C^{(i)} \, d\mu' \, d\varpi'}{R'^{\,i-2}} = o,$$

whenever i is an odd number. Thus all the equations we are considering, in which i is an odd number, are satisfied by the supposition that R′ is an even function of μ', $\sqrt{1-\mu'^2}$. Sin ϖ', $\sqrt{1-\mu'^2}$. Cos ϖ'.

It remains to consider the cases when i is an even number, viz.

$$\iint \frac{C^{(4)} \, d\mu' \, d\varpi'}{R'^2} = o$$

$$\iint \frac{C^{(6)} \, d\mu' \, d\varpi'}{R'^4} = o$$

$$+ \, \&c.$$

For this purpose the following theorem is premised, viz.

Theorem. If m, m', m'' denote any positive integer numbers, such that $m + m' + m''$ is less than i; then
$\iint \mu'^{m} . (\sqrt{1 - \mu'^2} . \text{Sin.}\, \varpi')^{m'} . (\sqrt{1 - \mu'^2} . \text{Cos.}\, \varpi')^{m''} . \text{C}^{(i)} d\mu' d\varpi' = 0$,
the integral being extended from $\mu' = 1$ to $\mu' = -1$, and from $\varpi' = 0$ to $\varpi' = 2\pi$.

As expressions of this kind have been very amply discussed, and as the theorem follows very readily from the properties generally known, I shall not stop to give the demonstration.

It follows from the theorem that the equation,

$$\iint \frac{\text{C}(4) d\mu' d\varpi'}{\text{R}'^2} = 0,$$

cannot be true, if $\frac{1}{\text{R}'^2}$ contain any even power of the quantities μ', $\sqrt{1 - \mu'^2} . \text{Sin.}\, \varpi'$, $\sqrt{1 - \mu'^2} . \text{Cos.}\, \varpi'$, above the square; or if it contain any product of two or more of the squares of the same quantities. Wherefore the most general value of $\frac{1}{\text{R}'^2}$, consistent with the above equation, is

$$\frac{1}{\text{R}'^2} = \text{A}\mu'^2 + \text{B}(1 - \mu'^2) \text{Sin.}^2 \varpi' + \text{C}(1 - \mu'^2) \text{Cos.}^2 \varpi'.$$

It may be observed, that this expression would not be more general by adding an absolute quantity, as D: for, since

$$\mu'^2 + (1 - \mu'^2) \text{Sin.}^2 \varpi' + (1 - \mu'^2) \text{Cos.}^2 \varpi' = 1,$$

such a quantity would blend itself with the other terms. But the same value of $\frac{1}{\text{R}'^2}$ will likewise satisfy all the equations,

$$\iint \frac{\text{C}^{(i)} d\mu' d\varpi'}{\text{R}'^{i-2}} = 0,$$

in which i is an even number. For, because

$$\frac{1}{\text{R}'^{i-2}} = (\text{A}\mu'^2 + \text{B}(1 - \mu'^2) \text{Sin.}^2 \varpi' + \text{C}(1 - \mu'^2) \text{Cos.}^2 \varpi')^{\frac{i-2}{1}},$$

it follows that the expansion of $\frac{1}{R^{i-2}}$ will produce no quantities in the integral except such as are evanescent by the theorem.

The most general value of $\frac{1}{R'^2}$, consistent with one of the conditions of the equilibrium, has now been found. If we write $\frac{1}{k^2}$, $\frac{1}{k'^2}$, $\frac{1}{k''^2}$, for A, B, C, we shall get,

$$\frac{1}{R'^2} = \frac{\mu'^2}{k^2} + \frac{(1-\mu'^2)\,\text{Sin.}^2\,\varpi'}{k'^2} + \frac{(1-\mu'^2)\,\text{Cos.}^2\,\varpi'}{k''^2};$$

an equation which belongs to an ellipsoid of which *k*, *k'*, *k''*, are the three semi-axes. It is therefore proved that a homogeneous fluid mass cannot be in equilibrio by the attraction of its particles, and a centrifugal force of rotation, unless its figure be included in the ellipsoids. But it is still to be shown that the same figure is consistent with the other condition of the equilibrium. For the sake of abridging, put

$$S = \mu'^2 + \frac{k^2}{k'^2}(1-\mu^2)\,\text{Sin.}^2\,\varpi' + \frac{k^2}{k''^2}(1-\mu'^2)\,\text{Cos.}^2\,\varpi';$$

then, $$R'^2 = \frac{k^2}{S}; \text{ and } R' = \frac{k}{\sqrt{S}}.$$

The value of Q being reduced to the first term of its expansion, we have,

$$Q = \int\!\!\int R'^2\,d\mu'\,d\varpi' = k^2.\int\!\!\int \frac{d\mu'\,d\varpi'}{S}:$$

and hence,

$$V(R) = Q + K.R^2 = k^2.\int\!\!\int \frac{d\mu'\,d\varpi'}{S} + KR^2.$$

Let this value be substituted in the equation (A), and we shall obtain,

$$R^2 = \frac{k^2\int\!\!\int\frac{d\mu'\,d\varpi'}{S} - C}{K + \frac{\omega}{2}(1-\mu^2)}$$

And the solution of the problem is now reduced to show that this formula is similar to the equation

$$R'^2 = \frac{k^2}{S}.$$

When this is done, the relation between the figure of the fluid mass and the given rotatory velocity will be found by making the two expressions of R and R′ coincide, so that both shall belong to the same surface.

In the first place, the integral in the numerator of the value of R^2 is a function of k, k', k''; and as any value may be assigned to C, the whole numerator may be regarded as an arbitrary quantity. The denominator is therefore all that remains to be considered. Now,

$$K = -\frac{2\pi}{3} + \iint \text{Log.}\, R' \times C^{(2)}\, d\mu'\, d\varpi';$$

and, since $\text{Log.}\, R' = \text{Log.}\, k - \frac{1}{2}\, \text{Log.}\, S$, we shall get,

$$K = -\frac{2\pi}{3} + \frac{1}{2}\iint - \text{Log.}\, S \times C^{(2)}\, d\mu'\, d\varpi',$$

because, $\text{Log.}\, k \times \iint C^{(2)}\, d\mu'\, d\varpi' = 0.$

Again, $C^{(2)} = \frac{1}{2.4} \cdot \frac{d^2.(1-\gamma^2)^2}{d\gamma^2} = \frac{3}{2}\gamma^2 - \frac{1}{2};$

and, $\gamma = \mu\mu' + \sqrt{1-\mu^2} \cdot \sqrt{1-\mu'^2}\,.\, \text{Cos.}\, (\varpi - \varpi')$;

or, if we write m, n, p, for μ, $\sqrt{1-\mu^2}\,.\, \text{Sin.}\, \varpi$, $\sqrt{1-\mu^2}\,.\, \text{Cos.}\, \varpi$, we shall obtain,

$$\gamma = m\mu + n\sqrt{1-\mu'^2}\,.\, \text{Sin.}\, \varpi + p\sqrt{1-\mu'^2}\,.\, \text{Cos.}\, \varpi':$$

and hence,

$$\begin{aligned}
K = -\frac{2\pi}{3} &+ \frac{m^2}{2}\iint - \left(\frac{3}{2}\mu'^2 - \frac{1}{2}\right) \text{Log.}\, S\,.\, d\mu'\, d\varpi' \\
&+ \frac{n^2}{2}\iint - \left\{\frac{3}{2}(1-\mu'^2)\, \text{Sin.}^2\, \varpi' - \frac{1}{2}\right\} \text{Log.}\, S.\, d\mu'\, d\varpi' \\
&+ \frac{p^2}{2}\iint - \left\{\frac{3}{2}(1-\mu'^2)\, \text{Cos.}^2\, \varpi' - \frac{1}{2}\right\} \text{Log.}\, S.\, d\mu'\, d\varpi' \\
&+ 3mn\iint - \mu'\sqrt{1-\mu'^2}\,.\, \text{Sin.}\, \varpi'\, \text{Log.}\, S.\, d\mu'\, d\varpi' \\
&+ 3mp\iint - \mu'\sqrt{1-\mu'^2}\,.\, \text{Cos.}\, \varpi'\, \text{Log.}\, S\,.\, d\mu'\, d\varpi' \\
&+ 3np\iint - (1-\mu'^2)\, \text{Sin.}\, \varpi'\, \text{Cos.}\, \varpi'\, \text{Log.}\, S\,.\, d\mu'\, d\varpi'.
\end{aligned}$$

But, because $\mu' = \text{Cos.}\,\theta'$, the three last integrals will become,

$$\iint \text{Sin.}^2\,\theta'\ \text{Cos.}\,\theta'\ \text{Sin.}\,\varpi'\ \text{Log.}\,S\,.\,d\theta'\,d\varpi'$$

$$\iint \text{Sin.}^2\,\theta\ \text{Cos.}\,\theta'\ \text{Cos.}\,\varpi'\ \text{Log.}\,S\,.\,d\theta'\,d\varpi'$$

$$\iint \text{Sin.}^3\,\theta'\ \text{Sin.}\,\varpi'\ \text{Cos.}\,\varpi'\ \text{Log.}\,S\,.\,d\theta'\,d\varpi' :$$

and, attending to the expression of S, it will follow that, in the two first integrals, if we suppose ϖ' to remain constant and θ' to vary from 0 to 180°; the fluxions will be equal, but will have different signs, at equal distances from 0 and 180°: wherefore the integrals, taken between the prescribed limits, are evanescent. In the third integral, if we suppose θ' to remain constant and ϖ' to vary from 0 to 360°, the fluxions will be equal, but will have different signs, at equal distances from 0 and 180° in the first semicircle: and at equal distances from 180° and 360° in the second semicircle: wherefore the whole integral is evanescent. Rejecting therefore the three last terms of the value of K, and representing the three remaining integrals by L, M, N, we shall get,

$$\begin{aligned} K + \tfrac{\omega}{2}(1-\mu^2) = {} & \left(\tfrac{L}{2} - \tfrac{3}{2}\pi\right) . \mu^2 \\ & + \left(\tfrac{M}{2} - \tfrac{3}{2}\pi + \tfrac{\omega}{2}\right) . (1-\mu^2)\ \text{Sin.}^2\,\varpi \\ & + \left(\tfrac{N}{2} - \tfrac{3}{2}\pi + \tfrac{\omega}{2}\right) . (1-\mu^2)\ \text{Cos.}^2\,\varpi . \end{aligned}$$

This is the denominator in the formula for R^2, and it is entirely similar to the expression of S. Wherefore the two values of R^2 and R'^2 are alike in point of form; and the figure of the fluid mass that corresponds to the given rotatory velocity will be determined by making them coincide.

We are now to conclude that a homogeneous fluid mass cannot be in equilibrio by the attraction of its particles and a centrifugal force of rotation, unless it have the figure of an ellipsoid; and farther, that an ellipsoid may be found that will fulfil all the conditions of the equilibrium, unless there be some cases in which the necessary relations between the figure and the given velocity of rotation, lead to equations that cannot be solved.

6. In order to apply the foregoing solution, it becomes necessary to compute the integrals L, M, N; or, in other words, to find the attractive forces of an ellipsoid upon a point in the surface. If we extend the problem generally to a point within or without the figure, it is attended with some difficulty; and it is usual to deduce the latter case from the former, which is more easily solved. There is however a great analogy between the two cases; or rather the distinction between them may be dispensed with; since the supposition of a point within the figure is equivalent to that of a point in the surface, which is the extreme case of a point without the figure. In this view the problem admits of a general solution deducible, by a short analysis, from the transformations used in this Paper.

Suppose that k, k', k'', represent the semi-axes of an ellipsoid; and let x, y, z, respectively parallel to the axes, denote the three co-ordinates of a point in the surface of the figure. Farther let a, b, c, be the co-ordinates of an attracted point without the figure; and conceive another ellipsoid, the surface of which passes through the attracted point, and which has its principal sections in the same planes with the principal sections of the given ellipsoid, and also the differences of the squares of its semi-axes h, h', h'', equal to the differences of the squares of k, k', k''; that is, $h'^2 - h^2 = k'^2 - k^2$, and $h''^2 - h^2 = k''^2 - k^2$. The equations of the two curve surfaces will thus be,

$$\frac{x^2}{k^2}+\frac{y^2}{k'^2}+\frac{z^2}{k''^2}=1$$

$$\frac{a^2}{h^2}+\frac{b^2}{h'^2}+\frac{c^2}{h''^2}=1,$$

$$h'^2-h^2=k'^2-k^2;\; h''^2-h^2=k''^2-k^2.$$

Again, as in the former part of this Paper, let

$$x=\mathrm{R}\,\mu' \qquad\qquad a=r\,\mu$$

$$y=\mathrm{R}\sqrt{1-\mu'^2}.\,\text{Sin.}\,\varpi', \qquad\qquad b=r\sqrt{1-\mu^2}\,\text{Sin.}\,\varpi$$

$$z=\mathrm{R}\sqrt{1-\mu'^2}.\,\text{Cos.}\,\varpi', \qquad\qquad c=r\sqrt{1-\mu^2}\,\text{Cos.}\,\varpi;$$

then R and *r* are respectively radii of the two ellipsoids.

Farther, assume

$$\frac{h}{k}\,x=\frac{h}{k}\,\mathrm{R}\,\mu'=\mathrm{R}'\,p'$$

$$\frac{h'}{k'}\,y=\frac{h'}{k'}\,\mathrm{R}\sqrt{1-\mu'^2}\,\text{Sin.}\,\varpi'=\mathrm{R}'\sqrt{1-p'^2}\,\text{Sin.}\,q'$$

$$\frac{h''}{k''}\,z=\frac{h''}{k''}\,\mathrm{R}\sqrt{1-\mu'^2}\,\text{Cos.}\,\varpi'=\mathrm{R}'\sqrt{1-p'^2}\,\text{Cos.}\,q'$$

$$\frac{k}{h}\,a=\frac{k}{h}\,r\,\mu=r'\,p$$

$$\frac{k'}{h'}\,b=\frac{k'}{h'}\,r\sqrt{1-\mu^2}\,\text{Sin.}\,\varpi=r'\sqrt{1-p^2}\,\text{Sin.}\,q$$

$$\frac{k''}{h''}\,c=\frac{k''}{h''}\,r\sqrt{1-\mu^2}\,\text{Cos.}\,\varpi=r'\sqrt{1-p^2}\,\text{Cos.}\,q.$$

From these formulæ we get

$$\mathrm{R}'^2=\frac{h^2}{k^2}\,x^2+\frac{h'^2}{k'^2}\,y^2+\frac{h''^2}{k''^2}\,z^2,$$

$$\mathrm{R}^2=x^2+y^2+z^2;$$

and hence,

$$\mathrm{R}'^2-\mathrm{R}^2=\left\{\frac{x^2}{k^2}+\frac{y^2}{k'^2}+\frac{z^2}{k''^2}\right\}(h^2-k^2)=h^2-k^2.$$

In like manner, it is shown that

$$r^2-r'^2=h^2-k.$$

Wherefore,

$$\mathrm{R}^2+r^2=\mathrm{R}'^2+r'^2.$$

And again,

$$a x + b y + c z = \mathrm{R}\, r \gamma = \mathrm{R}' r' \gamma' ;$$

$$\gamma = \mu \mu' + \sqrt{1-\mu^2} . \sqrt{1-\mu'^2} \text{ Cos. } (\varpi - \varpi')$$

$$\gamma' = p p' + \sqrt{1-p^2} . \sqrt{1-p'^2} \text{ Cos. } (q - q') .$$

Wherefore,

$$\mathrm{R}^2 - 2\, \mathrm{R}\, r \gamma + r^2 = \mathrm{R}'^2 - 2\, \mathrm{R}' r' \gamma' + r'^2 .$$

Farther, from the assumed equations we readily derive these values, viz.

$$\text{Tan. } \varpi' = \frac{h''}{k'} . \frac{k'}{k''} . \text{ Tan. } q',$$

$$\frac{\mu'}{\sqrt{1-\mu'^2}} = \frac{p'}{\sqrt{1-p'^2}} . \frac{k}{h} . \frac{k' h''}{\mathrm{M}},$$

$$\mathrm{M} = \sqrt{k'^2 h''^2 \text{ Sin.}^2 q' + h'^2 k''^2 \text{ Cos.}^2 q'}$$

$$\mathrm{R} \sqrt{1-\mu'^2} = \mathrm{R}' \sqrt{1-p'^2} . \frac{\mathrm{M}}{k' h''} .$$

And, if we now take the fluxions of the first and second of these formulæ; observing, in the second operation, to make q', and consequently M, constant; we shall get,

$$d \varpi' = d q' . \frac{k' k'' . h' h''}{\mathrm{M}^2},$$

$$\frac{d \mu'}{(1-\mu'^2)^{\frac{3}{2}}} = \frac{d p'}{(1-p'^2)^{\frac{3}{2}}} . \frac{k h' k''}{h \mathrm{M}}$$

$$\mathrm{R}^3 (1-\mu'^2)^{\frac{3}{2}} = \mathrm{R}'^3 (1-p'^2)^{\frac{3}{2}} . \frac{\mathrm{M}^3}{k'^3 h''^3} .$$

And, by combining these formulæ, we obtain,

$$\mathrm{R}^3 d \mu' d \varpi' = \frac{k k' k''}{h h' h''} . \mathrm{R}'^3 d p \; d q' .$$

But, in § 3 it has been shown, that,

$$-\left(\frac{d . \frac{\mathrm{V}(r)}{r^2}}{d r} \right) r^3 = \iint \frac{\mathrm{R}^3 d \mu' d \varpi'}{\sqrt{\mathrm{R}^2 - 2\, \mathrm{R}\, r \gamma + r^2}} :$$

and hence, by substitution,

$$-\left(\frac{d . \frac{\mathrm{V}(r)}{r^2}}{d r} \right) r^3 = \frac{k k' k''}{h h' h''} . \iint \frac{\mathrm{R}'^3 d p' d q'}{\sqrt{\mathrm{R}'^2 - 2\, \mathrm{R}' r' \gamma' + r'^2}} .$$

Now, in the equation of the ellipsoid of which k, k', k'' are the axes, if we substitute $\frac{R' p'}{h}$, $\frac{R' \sqrt{1 - p'^2}\, \text{Sin.}\, q'}{k'}$, $\frac{R' \sqrt{1 - p'}\, \text{Cos.}\, q'}{h''}$ for the equivalent quantities, $\frac{x}{k}$, $\frac{y}{k'}$, $\frac{z}{k''}$; we shall obtain,

$$\frac{1}{R'^2} = \frac{p'^2}{h^2} + \frac{(1 - p'^2)\, \text{Sin.}^2\, q'}{h'^2} + \frac{(1 - p'^2)\, \text{Cos.}^2\, q'}{h''}.$$

And, by a like procedure in the other ellipsoid, we obtain,

$$\frac{1}{r'^2} = \frac{p^2}{k^2} + \frac{(1 - p^2)\, \text{Sin.}^2\, q}{k'^2} + \frac{(1 - p^2)\, \text{Cos.}^2\, q}{h''^2}.$$

Thus R' is a radius of the ellipsoid that passes through the attracted point, and r' a radius of the given ellipsoid which is entirely within the first figure. The last integral may therefore be developed in a series of the ascending powers of r': and then, applying the same reasoning as in the former case of the developement of Q,* it will be found that all the terms are evanescent, except the first. Thus the general term of the expansion is,

$$r'^i \times \iint \frac{C^{(i)}\, dp'\, dq'}{R'^{i-2}}:$$

and, when i is an odd number, this integral is equal to zero; because the increment at any point on the surface of the sphere, is just equal to the decrement at the point diametrically opposite: and, when i is an even number, it is evanescent; because $\frac{1}{R'^{i-2}}$ contains no terms except such as are evanescent in the integral, according to the theorem in § 5. Wherefore, the integral being reduced to the first term of its expansion, we get,

$$-\left(\frac{d \cdot \frac{V(r)}{r^3}}{dr}\right) r^3 = \iint R''\, dp'\, dq';$$

p' and q' being taken between the same limits, as μ' and ϖ'.

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Now, we have

$$R'^2 = \frac{h^2}{p'^2 + \frac{h^2}{h'^2}(1 - p'^2)\,\text{Sin.}^2 q' + \frac{h^2}{h''^2}(1 - p'^2)\,\text{Cos.}^2 q'};$$

or, which is the same thing,

$$R'^2 = \frac{h^2 h'^2 h''^2}{h''^2 (h^2 + e^2 p'^2)\,\text{Sin.}^2 q' + h'^2 (h^2 + e'^2 p'^2)\,\text{Cos.}^2 q'},$$

$$h'^2 - h^2 = e^2,\ h''^2 - h^2 = e'^2;$$

wherefore

$$\iint R'^2\, dp'\, dq' = \iint \frac{h^2 h'^2 h''^2 \,.\, dp'\, dq'}{h''^2 (h^2 + e^2 p'^2)\,\text{Sin.}^2 q' + h'^2 (h^2 + e'^2 p'^2)\,\text{Cos.}^2 q'}.$$

This expression is now integrable with regard to q': and we get, between the limits $q' = o$ and $q' = 2\pi$,

$$\iint R'^2\, dp'\, dq' = 2\pi \,.\, h h' h'' . \int \frac{h\, dp'}{\sqrt{(h^2 + e^2 p'^2) \,.\, (h^2 + e'^2 p'^2)}}.$$

The integral now found increases as much, while p' decreases from 1 to o, as it does, while p' decreases from o to -1: wherefore the whole value will be the same, if we make p' vary between the limits 1 and o; and then double the result: thus,

$$\iint R'^2\, dp'\, dq' = 4\pi \,.\, h h' h'' . \int \frac{h\, dp'}{\sqrt{(h^2 + e^2 p'^2) \,.\, (h^2 + e'^2 p'^2)}}.$$

Finally put $\frac{p'}{h} = \frac{1}{x}$; then we get,

$$\iint R'^2\, dp'\, dq' = 4\pi \,.\, h h' h'' . \int \frac{-dx}{\sqrt{(x^2 + e^2)(x^2 + e'^2)}};$$

the limits of x being, $x = h$ and $x = \infty$.

Wherefore, by substitution, we get,

$$-\left(\frac{d \,.\, \frac{V(r)}{r^2}}{dr}\right) r^3 = 4\pi \,.\, k k' k'' . \int \frac{-dx}{\sqrt{(x^2 + e^2)(x^2 + e'^2)}}.$$

Now, multiply by $-\frac{dr}{r^3}$, and then integrate; and, having multiplied by r^2 after the integration, we shall obtain,

$$V(r) = 2\pi \,.\, k k' k'' . \int \frac{-dx}{\sqrt{(x^2 + e^2)(x^2 + e'^2)}}$$

$$- 2\pi \,.\, k k' k'' .\, r^2 . \int \frac{1}{r^2} \cdot \frac{-dx}{\sqrt{(x^2 + e^2)(x^2 + e'^2)}}.$$

In the expression under the sign of integration, r increases from its value at the given attracted point till it becomes infinitely great; the angles which it makes with the axes of the co-ordinates remaining constantly the same. But if we substitute the values of a, b, c in the equation,

$$\frac{a^2}{h^2}+\frac{b^2}{h'^2}+\frac{c^2}{h''^2}=1;$$

we shall get,

$$\frac{1}{r^2}=\frac{\mu^2}{h^2}+\frac{(1-\mu^2)\,\text{Sin.}^2\,\varpi}{h^2+e^2}+\frac{(1-\mu^2)\,\text{Cos.}^2\,\varpi}{h^2+e'^2};$$

and, by writing x^2 for h^2,

$$\frac{1}{r^2}=\frac{\mu^2}{x^2}+\frac{(1-\mu^2)\,\text{Sin.}^2\,\varpi}{x^2+e^2}+\frac{(1-\mu^2)\,\text{Cos.}^2\,\varpi}{x^2+e'^2};$$

in which expression, μ and ϖ remaining the same, x will vary from h to be infinite, while r increases from its value at the given attracted point to be infinite. Wherefore, by substitution, we get,

$$\begin{aligned}\text{V}(r)=\;&2\pi\,.\,kk'k''.\int\frac{-dx}{(x^2+e^2)^{\frac{1}{2}}(x^2+e'^2)^{\frac{1}{2}}}\\ &-2\pi\,.\,kk'k''.\,r^2\mu^2.\int\frac{-dx}{x^2(x^2+e^2)^{\frac{1}{2}}(x^2+e'^2)^{\frac{1}{2}}}\\ &-2\pi\,.\,kk'k''.\,r^2(1-\mu^2)\,\text{Sin.}^2\,\varpi.\int\frac{-dx}{(x^2+e^2)^{\frac{3}{2}}(x^2+e'^2)}\\ &-2\pi\,.\,kk'k''.\,r^2(1-\mu^2)\,\text{Cos.}^2\,\varpi.\int\frac{-dx}{(x^2+e^2)^{\frac{1}{2}}(x+e'^2)^{\frac{3}{2}}}.\end{aligned}$$

Let M denote the mass of the ellipsoid, then $\text{M}=\frac{4}{3}\pi\,.\,kk'k''$: wherefore, by substituting a, b, c, for the equivalent quantities, we finally get,

$$\begin{aligned}\text{V}(r)=\;&\frac{3\text{M}}{2}.\int\frac{-dx}{(x^2+e^2)^{\frac{1}{2}}(x^2+e'^2)^{\frac{1}{2}}}\\ &-a^2.\frac{3\text{M}}{2}.\int\frac{-dx}{x^2(x^2+e^2)^{\frac{1}{2}}(x^2+e'^2)^{\frac{1}{2}}}\\ &-b^2.\frac{3\text{M}}{2}.\int\frac{-dx}{(x^2+e^2)^{\frac{3}{2}}(x+e'^2)^{\frac{1}{2}}}\\ &-c^2.\frac{3\text{M}}{2}.\int\frac{-dx}{(x^2+e^2)^{\frac{1}{2}}(x^2+e'^2)^{\frac{3}{2}}}.\end{aligned}$$

To this expression we must join the equation of the surface of the ellipsoid that passes through the attracted point, viz.

$$\frac{a^2}{h^2} + \frac{b^2}{h^2 + e^2} + \frac{c^2}{h^2 + e'^2} = 1,$$

by means of which h, the limit of x in the several integrals, is to be determined. When the attracted point is in the surface of the given ellipsoid, it is plain that $h = k$; and the limit of x is, therefore, one of the semi-axes.

Thus an expression of $V(r)$ has been found, that is general for all positions of the attracted point; nothing more being requisite than to determine the limit of x in every particular case. The several integrals are closely connected with one another; they are in forms well known to geometers, and susceptible of many transformations; but, in a general solution, it seems most simple to leave the expression as it is above exhibited.

But although a general expression of $V(r)$ has been found, yet it does not immediately make known the attractive forces acting upon a point. These forces, estimated in directions parallel to the co-ordinates, are represented by the partial fluxions of $V(r)$ relatively to the co-ordinates; but, in performing the operations, it must be observed that x is a function of r, and consequently of the co-ordinates. Thus the attractions of the ellipsoid, respectively parallel to a, b, c, are equal to,

$$-\frac{d.V(r)}{da} - \frac{d.V(r)}{dx}\cdot\frac{dx}{da}$$

$$-\frac{d.V(r)}{db} - \frac{d.V(r)}{dx}\cdot\frac{dx}{db}$$

$$-\frac{d.V(r)}{dc} - \frac{d.V(r)}{dx}\cdot\frac{dx}{dc}.$$

But, according to the foregoing value of $V(r)$,

$$\frac{d.V(r)}{dx} = \frac{\frac{3}{4}M}{\sqrt{(x^2+e^2)(x^2+e'^2)}}\cdot\left\{-1 + \frac{a^2}{x^2} + \frac{b^2}{x^2+e^2} + \frac{c^2}{x^2+e'^2}\right\}:$$

and as this quantity is to be valued at the limit, or when $x = h$, we have $\frac{d \cdot V(r)}{dx} = 0$. Wherefore the expressions of the attractive forces are reduced to, $-\frac{d \cdot V(r)}{da}$, $-\frac{d \cdot V(r)}{db}$, $-\frac{d \cdot V(r)}{dc}$; that is, to the partial fluxions of $V(r)$, supposing that x is independent of the co-ordinates, a, b, c.

The oblate ellipsoid of revolution, corresponds to the supposition, $e^2 = e'^2$; and, in this case, we get,

$$V(r) = \tfrac{3}{2} M \cdot \int \frac{-dx}{x^2 + e^2} - a^2 \cdot \tfrac{3}{2} M \cdot \int \frac{-dx}{x^2 (x^2 + e^2)}$$
$$- (b^2 + c^2) \cdot \tfrac{3}{2} M \cdot \int \frac{-dx}{(x^2 + e^2)^2} :$$

the equation for finding h, the limit of x, being,

$$\frac{a^2}{h^2} + \frac{b^2 + c^2}{h^2 + e^2} = 1.$$

And, when the attracted point is in the surface of the oblate spheroid of revolution, h is equal to k; and, if we put $\lambda = \frac{e}{k}$, we get,

$$V(r) = \frac{3M}{2k} \cdot \frac{\text{Arc. Tan.}\,\lambda}{\lambda} - a^2 \cdot \frac{3M}{2k^3} \cdot \frac{\lambda - \text{Arc. Tan.}\,\lambda}{\lambda^3} - (b^2 + c^2) \cdot \frac{3M}{4k^3} \cdot \frac{\text{Arc. Tan.}\,\lambda - \frac{\lambda}{1 + \lambda^2}}{\lambda^3}.$$

7. It may not be improper to apply the foregoing solution to find the relation between the figure and the velocity of rotation in the case of an oblate ellipsoid of revolution. As it has been proved that the supposed figure will satisfy one of the conditions of equilibrium,* nothing more is requisite than to employ the other condition, namely, that contained in the equation (A), to determine the relation sought.

Let k be the semi-axis of revolution, and $\sqrt{k^2 + e^2}$, the radius of the equator; if a, b, c be three rectangular co-ordinates of a point in the surface, a being parallel to k, the equation of the figure will be,

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$$\frac{a^2}{k^2} + \frac{b^2 + c^2}{k^2 + e^2} = 1.$$

Put $a^2 = R^2 \mu^2$; $b^2 + c^2 = R^2 (1 - \mu^2)$; $e^2 = k^2 \times \lambda^2$; then, by substitution, we shall get,

$$R^2 (1 + \lambda^2 \mu^2) = k^2 (1 + \lambda^2).$$

Again, from the formula (A), we get

$$V(R) + R^2 . \frac{\omega}{2} (1 - \mu^2) = C,$$

ω being the centrifugal force at the distance 1 from the axis of rotation. This equation must be made identical with the former one, and for this purpose we must substitute in it the value of V (R) reduced to a proper form. Now, M being the mass of the spheroid, we have

$$M = \frac{4\pi}{3} k (k^2 + e^2) = \frac{4\pi}{3} k^3 (1 + \lambda^2);$$

and for the sake of abridging, if we put,

$$A = \frac{\lambda - \text{Arc. Tan. } \lambda}{\lambda^3}$$

$$B = \frac{\text{Arc. Tan. } \lambda - \frac{\lambda}{1 + \lambda^2}}{2 \lambda^3};$$

and likewise attend to the values of a^2 and $b^2 + c^2$, we shall get by the formula in § 6,

$$V(R) = 2\pi k^2 . \frac{(1 + \lambda^2) \text{ Arc. Tan. } \lambda}{\lambda^2} - R^2 . 2\pi \left\{ (1 + \lambda^2) B + (1 + \lambda^2)(A - B) . \mu^2 \right\}:$$

and hence, by substitution, the equation we are considering, when brought to the same form of expression as the first one, will become,

$$R^2 . \left\{ 1 + \frac{(1 + \lambda^2)(A - B) + \frac{\omega}{4\pi}}{(1 + \lambda^2) B - \frac{\omega}{4\pi}} . \mu^2 \right\} = \frac{k^2 (1 + \lambda)^2 \frac{\text{Arc. Tan. } \lambda}{\lambda} - \frac{C}{2\pi}}{(1 + \lambda^2) B - \frac{\omega}{4\pi}}.$$

By comparing the two equations, it will appear that the only condition necessary to make them identical, is this, viz.

$$\frac{(1+\lambda^2)(A-B)+\frac{\omega}{4\pi}}{(1+\lambda^2)B-\frac{\omega}{4\pi}}=\lambda^2;$$

for the terms on the right-hand sides will be made to coincide by giving a proper value to the arbitrary quantity C. Hence,

$$\frac{\omega}{4\pi}=(1+\lambda^2)B-A.$$

Now put,

$$\frac{\omega}{\frac{4}{3}\pi}=q;$$

then, restoring the values of A and B, we shall obtain,

$$\frac{2}{9}q=\frac{1}{3}-\left(\frac{1}{3}+\frac{1}{\lambda^2}\right)\cdot\left(1-\frac{\text{Arc. Tan.}\,\lambda}{\lambda}\right).$$

From this formula it appears that $q=0$, both when λ is equal to zero and when it is infinitely great. There is therefore no rotatory motion in either of the extreme cases, when the oblateness is nothing, and when it is infinite; or when the fluid mass is a sphere, and when it is a circular sheet spread out in the plane of the equator. In order to discover whether q is evanescent in any other circumstances, put Tan. $\varphi=\lambda$; then

$$\frac{2}{9}q=\frac{1}{3}-\left(\frac{1}{\text{Sin.}^2\varphi}-\frac{2}{3}\right)\left(1-\frac{\varphi\,\text{Cos.}\,\varphi}{\text{Sin.}\,\varphi}\right);$$

or, in a series,

$$q=\frac{2}{5}\,\text{Sin.}^2\varphi+\frac{1.2}{5\cdot7}\,\text{Sin.}^4\varphi-\frac{1.2.4.6}{5\cdot7\cdot9\cdot11}\,\text{Sin.}^8\varphi$$
$$-\frac{1.2.4.6.8}{5\cdot7\cdot9.11.13}\,\text{Sin.}^{10}\varphi$$
$$-\&c.$$

Now this series being evanescent both when Sin. $\varphi=0$, and when Sin. $\varphi=1$, it follows that, for every other value of Sin. φ, q will be positive; and hence it will first increase from zero to a maximum, and then decrease to the first limit. If we seek the value of λ that will make q a maximum, we shall find this equation, viz.

$$\text{Arc. Tan. } \lambda = \frac{\lambda}{1+\lambda^2} \cdot \frac{9+7\lambda^2}{9+\lambda^2};$$

from which λ comes out equal to 2·5292. And hence $\sqrt{1+\lambda^2}$, which is the proportion of the equatorial diameter to the polar axis, is equal to 2.7197.

From all this it follows, that if a homogeneous mass of fluid in equilibrio, at rest, and consequently of a spherical figure, begin to revolve about a diameter, it will become more and more oblate as the velocity of rotation increases, till the equatorial diameter have to the polar axis the proportion of 2·7197 to 1: arrived at this point the rotatory velocity must decrease, in order that the fluid in equilibrio continue to have the figure of an ellipsoid of revolution with increasing oblateness; in so much that while the oblateness tends to be infinite, and the fluid to become a circular sheet in the plane of the equator, the velocity of rotation continually approaches to zero.

As the oblateness increases without ceasing, there is but one rotatory velocity with which a spheroid of a given figure will be in equilibrio.

But when a fluid mass is to revolve in a given time, and the figure that will maintain the equilibrium is sought, there are two solutions, if the proposed rotation be within the maximum, and one only, when it reaches that limit.

When the rotatory velocity is greater than the maximum, the equilibrium cannot take place: for, on the one hand, the proposed rotation is inconsistent with the figure of an ellipsoid; and, on the other, it has been proved, that a homogeneous fluid cannot be in equilibrio unless it have that figure. In this case, therefore, the fluid would first extend itself, and flatten to a certain degree with a decreasing velocity of rotation, and then oscillate back with an increasing rotatory motion. But the tenacity of the particles would gradually diminish, and finally destroy, the oscillations of the fluid; which would therefore ultimately settle in one of the figures of equilibrium; that is, in an elliptical spheroid of revolution having the equatorial diameter more than 2·7179 times the axis of revolution.

When the oblate figures are little different from spheres, as in the case of the planets, λ, which is equal to the excentricity of the meridian divided by half the polar axis, is so small that we may consider λ^2 as equal to $\text{Sin.}^2\,\phi$, and may reject all the powers of these two quantities. The series for q will thus be reduced to its first term, viz.

$$q = \frac{2}{5}\,\text{Sin.}^2\,\phi = \frac{2}{5}\,\lambda^2.$$

But the polar axis is to the equatorial diameter as 1 to $\sqrt{1+\lambda^2}$, or as 1 to $1+\frac{1}{2}\lambda^2$: wherefore the same proportion is equal to that of 1 to $1+\frac{5}{4}q$.

Again, we have $$q = \frac{\omega}{\frac{4}{3}\pi};$$

now, $\frac{4}{3}\pi$ being the mass of a sphere of which the density and the radius are each equal to unit, it will represent the gravitation at the surface; and, if we suppose the same sphere to revolve with the given rotatory velocity, ω will be the centrifugal force at the equator. Wherefore q is the proportion of the centrifugal force at the equator to the gravity; a proportion which remains the same in all spheres that have the same density and the same velocity of rotation, because both the quantities increase as the radius of the sphere. Hence, in a planet of small oblateness, the value of q to the degree of approximation mentioned, is equal to the proportion of the centrifugal force to the gravitation at the equator; and the proportion of 1 to $1 + \frac{5}{4} q$ is equal to that of the polar axis to the equatorial diameter.

8. In the determination of the equilibrium of a homogeneous fluid mass investigated in this Paper, two conditions are found necessary when the particles are endowed with attractive powers; whereas, in the usual solution of the problem one only is deemed sufficient, namely, that contained in equation (A), which expresses that the resultant of the accelerating forces acting upon the particles in the outer surface shall be every where perpendicular to that surface. It is extremely remarkable that, of the two conditions, the one which is usually omitted, alone and without reference to the other, ascertains the kind of the figures of equilibrium.

M'LAURIN first proved synthetically that the ellipsoid, whatever be the degree of oblateness, fulfils all the conditions requisite for maintaining the equilibrium of a homogeneous fluid mass that revolves about an axis. If therefore the equation (A) were alone sufficient for the equilibrium, the ellipsoid must be deducible from it, not in particular suppositions and approximately, but generally, and by an accurate process of reasoning. But this has not been accomplished, nor even attempted, by any geometer. No application has hitherto been made of the hydrostatical theory, except in the case of spheroids little different from spheres.

If a homogeneous fluid of a spherical form at rest, and consequently in equilibrio, begin to revolve about a diameter with a rotatory velocity causing a centrifugal force at the equator, very small in proportion to the gravitation; the sphere will acquire a small degree of oblateness at the poles, and the new surface of equilibrium must come under the equation (A). Now, from these considerations alone, without any reference to the other condition of equilibrium, it has been proved by LEGENDRE and LAPLACE, that the particles of the fluid will arrange themselves very nearly in an ellipsoid of revolution, the deviation being proportional to the square and higher powers of the oblateness. But, as the coincidence of the true figure of equilibrium with the ellipsoid is not exact, the result seems to be inconsistent with what M'LAURIN has so ably and elegantly demonstrated to be true. This argument will acquire greater force, and will even become conclusive, against the theory which makes the equation (A) the only condition of the equilibrium, if we consider the oblateness as a finite quantity, and push the approximation so as to take in the square and higher powers:* for, by this procedure, we obtain a series of figures in which the ellipsoid is not included.

There is a great analogy between the modern theory of spheroids little different from spheres, and the assumption of NEWTON, who tacitly supposed that the fluid sphere, in the nascent change of its form, will become, either exactly or very nearly, an elliptical spheroid, oblate at the poles. Both views of the subject leave us in ignorance of the exact form of the surface of equilibrium, although, in the supposed circumstances, it is proved in the one, and assumed in the other, that it is nearly an ellipsoid.

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* Mec. Celeste, Vol. ii, p. 105, No. 37.

The figures of the earth and of the planets being entirely deduced from the properties of spheroids little different from spheres, it may not be improper to conclude this Paper with a short exposition of a theory that occupies so conspicuous a place in the celestial mechanics, and which is so intimately connected with the subject we have been discussing.

For this purpose resume the expansion of $V(r)$ already given in § 3, viz.

$$\begin{aligned} V(r) = & \iint R'^2 \, d\mu' \, d\varpi' + r \iint R' . C^{(1)} \, d\mu' \, d\varpi' \\ & + r^2 . \left\{ - \frac{2\pi}{3} + \iint \text{Log.} \, R' . C^{(2)} \, d\mu' \, d\mu' \right\} \\ & - \frac{r^3}{1} \iint \frac{C^{(3)} \, d\mu' \, d\varpi'}{R'} . \\ & \quad \vdots \\ & - \frac{r^i}{i-2} \iint \frac{C^{(i)} \, d\mu' \, d\varpi'}{R'^{i-2}} \\ & \quad \text{\&c.} \end{aligned}$$

The spheroid being nearly a sphere, we may suppose $R' = a . (1 + \alpha . y')$; α being a small coefficient of which the square and other powers are to be neglected; and y' a function of the angles that determine the position of R'. The expansion supposes that the attracted point is within the spheroid; but it will apply when the same point is in the surface, in which case, $r = R = a(1 + \alpha . y)$. Now, let the values of R and R' be substituted, and we shall obtain,

$$\begin{aligned} V(R) = & \iint \frac{a^2}{2} (1 + 2\alpha y') \, d\mu' \, d\varpi' - \frac{2\pi}{3} a^2 (1 + 2\alpha y) \\ & + a^2 \alpha . \iint y' . C^{(1)} \, d\mu' \, d\varpi' \\ & + a^2 \alpha . \iint y' . C^{(2)} \, d\mu' \, d\varpi' \\ & + a^2 \alpha . \iint y' . C^{(3)} \, d\mu' \, d\varpi' \\ & + \text{\&c.} \end{aligned}$$

This expression is to be substituted in the equation (A): and it is to be observed that $\iint \frac{a^2}{2}\, d\mu'\, d\varpi' = 2\pi a^2$; and that ω is of the same order with α. Hence we get,

$$\begin{aligned} C = \tfrac{4\pi}{3} a^2 - \tfrac{4\pi}{3} a^2 y + a^2 \alpha . \iint y'\, d\mu'\, d\varpi' & \\ + a^2 \alpha . \iint y' . C^{(1)}\, d\mu'\, d\varpi' & \\ + a^2 . \left\{ \alpha . \iint y' . C^{(2)}\, d\mu'\, d\varpi' + \tfrac{\omega}{2}(1-\mu^2) \right\} & \\ + a^2 \alpha . \iint y' . C^{(3)}\, d\mu'\, d\varpi' & \\ + \text{\&c.} & \end{aligned}$$

This is the approximate equation of the surface, when the equation (A) is alone taken into account; and it is to be proved that this equation cannot subsist unless it belong to an elliptical spheroid of revolution.

In the first place, the nature of the function y must depend upon the integrals by which its value is expressed. But all the integrals are independent of the angles that determine the position of the attracted point in the surface, unless so far as those angles enter into the expressions $C^{(1)}$, $C^{(2)}$, $C^{(3)}$, &c. which are all functions of γ. Now γ is a function of μ, $\sqrt{1-\mu^2}\,\text{Sin.}\,\varpi$, $\sqrt{1-\mu^2}\,.\,\text{Cos.}\,\varpi$: and hence it follows, that y and y' are functions of three rectangular co-ordinates of a point in the surface of a sphere.

In the second place, every function of three rectangular co-ordinates is susceptible of an arrangement, by which it will be converted into a series of the same integrals contained in the foregoing equation. Let

$$f = \sqrt{1 - 2e\gamma + e^2}:$$

then, as is well known, we shall have,

$$4\pi y = \iint \left(\frac{1}{f} + 2\epsilon \frac{d.\frac{1}{f}}{d\epsilon} \right) y'\, d\mu'\, d\varpi',$$

provided we make $\epsilon = 1$, after the integration. Now for $\frac{1}{f}$ substitute its developement,* and make $\epsilon = 1$: then,

$$\begin{aligned} 4\pi y = & \iint y'\, d\mu'\, d\varpi' \\ & + 3\iint y'.\, C^{(1)}\, d\mu'\, d\varpi' \\ & + 5\iint y'.\, C^{(2)}\, d\mu'\, d\varpi' \\ & \quad \vdots \\ & + (2i+1)\iint y'.\, C^{(1)}\, d\mu'\, d\varpi' \\ & + \&c. \end{aligned}$$

This expression is identical when y and y' are functions of three rectangular co-ordinates. It is analytically true of every function that can be algebraically transformed into an expression of three rectangular co-ordinates; and thus it may be said to comprehend every function of two variable angles.

We have now obtained two expressions of y in the same quantities. But it is easy to prove that the same function can be so expressed only one way. The two values of y must therefore be identical; and all the terms that cannot be made to coincide, must be evanescent. Hence we obtain

$$\begin{aligned} o &= \iint y'.\, C^{(1)}\, d\mu'\, d\varpi' \\ o &= \iint y'.\, C^{(3)}\, d\mu'\, d\varpi' \\ & \quad \vdots \\ o &= \iint y'.\, C^{(i)}\, d\mu'\, d\varpi' \\ & \quad \&c. \end{aligned}$$

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* Section 2.

from which it is easy to infer that the most general value of y' is thus expressed, viz.

$$p' = \mathrm{A} \,.\, \mu'^2 + \mathrm{B}\,(1 - \mu'^2)\,\mathrm{Sin.}^2\,\varpi' + \mathrm{C}\,(1 - \mu'^2)\,\mathrm{Cos.}^2\,\varpi'.$$

It deserves to be remarked, that the equations just found are the very same that result from the second condition of equilibrium, when, for R$'$, we substitute $a\,(1 + \alpha \,.\, y')$, and neglect the powers of α.

Now, leaving out the evanescent terms, the two foregoing expressions will become,

$$\mathrm{C} = \tfrac{4\pi}{3}\,a^3 - \tfrac{4\pi}{3}\,a^3 y + a^3 \alpha \,.\, \textstyle\iint y'\,d\mu'\,d\varpi'$$
$$+\, a^3 \,.\, \left\{\alpha \textstyle\iint y' \,.\, \mathrm{C}^{(2)}\,d\mu'\,d\varpi' + \tfrac{\omega}{2}\,(1 - \mu^2)\right\}$$
$$4\pi y = \textstyle\iint y'\,d\mu'\,d\varpi' + 5 \iint y' \,.\, \mathrm{C}^{(2)}\,d\mu'\,d\varpi' :$$

and farther, if we exterminate the integral containing $\mathrm{C}^{(2)}$ from the first, we shall obtain,

$$\mathrm{C} = \tfrac{4\pi}{3}\,a^3 + \tfrac{4}{5}\,a^3 \alpha \,.\, \textstyle\iint y'\,d\mu'\,d\varpi'$$
$$-\, a^3 \,.\, \left\{\tfrac{8\pi}{15} \,.\, \alpha y - \tfrac{\omega}{2}\,(1 - \mu^2)\right\}$$
$$4\pi y = \textstyle\iint y'\,d\mu'\,d\varpi' + 5 \,.\, \iint y' \,.\, \mathrm{C}^{(2)}\,d\mu'\,d\varpi'.$$

The first of these equations proves that y is a function of μ only, and that the spheroid sought is one of revolution. The second is satisfied by putting,

$$y = f\,(1 - \mu^2)$$
$$y' = f\,(1 - \mu'^2) :$$

wherefore, by substituting these values, the first will become,

$$\mathrm{C} = \tfrac{4\pi}{3}\,a^3\,(1 + \tfrac{8}{5}\,\alpha f) - a^3 \left(\tfrac{8\pi}{15} \,.\, \alpha f - \tfrac{\omega}{2}\right)(1 - \mu^2).$$

Hence we finally get,

$$q = \frac{\omega}{\frac{4}{3}\pi}$$

$$\alpha f = \tfrac{5}{4} q$$

$$C = \frac{4\pi}{3} a^2 . (1 + 2q)$$

$$\alpha y = \alpha f (1 - \mu^2) = \tfrac{5}{4} q (1 - \mu^2)$$

$$a (1 + \alpha y) = a \left\{ 1 + \tfrac{5}{4} q (1 - \mu^2) \right\}.$$

Such is the method of investigation for which we are indebted to Legendre and Laplace in its fundamental principles: for, when all the operations necessary for applying it extensively and readily are fully explained, it becomes a great branch of analysis. The result is no more than an approximation, both on account of the quantities omitted, and because no attention is paid to one of the conditions of equilibrium. Considering the near approach of all the planets to the spherical form, the method of calculation may be deemed sufficiently accurate for determining the figure of the fluids that cover their surfaces; but it is not the less necessary to place the physical theory on a clear and sure foundation. As the subject is usually treated, there is an obscurity, and a want of evidence, arising from the inconsistency between the hydrostatical theory and what is proved by M'Laurin, which is extremely embarrassing, but which entirely disappears, when we take into account all the physical conditions requisite to maintain the equilibrium of a homogeneous fluid mass that revolves upon an axis.

J. IVORY.

November 2, 1823.

VI. *On the corrosion of copper sheeting by sea water, and on methods of preventing this effect; and on their application to ships of war and other ships. By Sir* HUMPHRY DAVY, *Bart. Pres. R. S.*

Read January 22, 1824.

1. THE rapid decay of the copper sheeting of His Majesty's ships of war, and the uncertainty of the time of its duration, have long attracted the attention of those persons most concerned in the naval interests of the country. Having had my enquiries directed to this important object by the Commissioners of the Navy Board, and a Committee of the Royal Society having been appointed to consider of it, I entered into an experimental investigation of the causes of the action of sea water upon copper. In pursuing this investigation, I have ascertained many facts which I think not unworthy of the notice of the Royal Society, as they promise to illustrate some obscure parts of electro-chemical science; and likewise seem to offer important practical applications.

2. It has been generally supposed that sea water had little or no action on pure copper, and that the rapid decay of the copper on certain ships was owing to its impurity. On trying, however, the action of sea water upon two specimens of copper, sent by JOHN VIVIAN, Esq. to Mr. FARADAY for analysis, I found the specimen which appeared absolutely pure, was acted upon even more rapidly than the specimen which contained alloy: and, on pursuing the enquiry with specimens of various kinds of copper which had been collected by the

Navy Board, and sent to the Royal Society, and some of which had been considered as remarkable for their durability, and others for their rapid decay, I found that they offered very inconsiderable differences only in their action upon sea water; and, consequently, that the changes they had undergone must have depended upon other causes than the absolute quality of the metal.

3. To enable persons to understand fully the train of these researches, it will be necessary for me to describe the nature of the chemical changes taking place in the constituents of sea water by the agency of copper.

When a piece of polished copper is suffered to remain in sea water, the first effects observed are, a yellow tarnish upon the copper, and a cloudiness in the water, which take place in two or three hours: the hue of the cloudiness is at first white; it gradually becomes green. In less than a day a bluish-green precipitate appears in the bottom of the vessel, which constantly accumulates; at the same time that the surface of the copper corrodes, appearing red in the water, and grass-green where it is in contact with air. Gradually carbonate of soda forms upon this grass-green matter; and these changes continue till the water becomes much less saline.

The green precipitate, when examined by the action of solution of ammonia and other tests, appears principally to consist of an insoluble compound of copper, (which may be considered as a hydrated sub-muriate) and hydrate of magnesia.

According to the views which I developed fourteen years ago, of the nature of the compounds of chlorine, and which are now generally adopted, it is evident that soda and magnesia cannot appear in sea water by the action of a metal, unless in consequence of an absorption or transfer of oxygene. It was therefore necessary for these changes, either that water should be decomposed, or oxygene absorbed from the atmosphere. I found that no hydrogene was disengaged, and consequently no water decomposed: necessarily, the oxygene of the air must have been the agent concerned, which was made evident by many experiments.

Copper in sea water deprived of air by boiling or exhaustion, and exposed in an exhausted receiver or an atmosphere of hydrogene gas, underwent no change; and an absorption in atmospherical air was shown when copper and sea water were exposed to its agency in close vessels.

4. In the Bakerian Lecture for 1806, I have advanced the hypothesis, that chemical and electrical changes may be identical, or dependent upon the same property of matter: and I have farther explained and illustrated this hypothesis in an elementary work on chemistry, published in 1812. Upon this view, which has been adopted by M. BERZELIUS and some other philosophers, I have shown that chemical attractions may be exalted, modified, or destroyed, by changes in the electrical states of bodies; that substances will only combine when they are in different electrical states; and that, by bringing a body naturally positive artificially into a negative state, its usual powers of combination are altogether destroyed; and it was by an application of this principle that, in 1807, I separated the bases of the alkalies from the oxygene with which they are combined, and preserved them for

examination; and decomposed other bodies formerly supposed to be simple.

It was in reasoning upon this general hypothesis likewise, that I was led to the discovery which is the subject of this Paper.

Copper is a metal only weakly positive in the electrochemical scale; and, according to my ideas, it could only act upon sea water when in a positive state; and, consequently, if it could be rendered slightly negative, the corroding action of sea water upon it would be null; and whatever might be the differences of the kinds of copper sheeting and their electrical action upon each other, still every effect of chemical action must be prevented, if the whole surface were rendered negative. But how was this to be effected? I at first thought of using a Voltaic battery; but this could be hardly applicable in practice. I next thought of the contact of zinc, tin, or iron: but I was for some time prevented from trying this, by the recollection that the copper in the Voltaic battery, as well as the zinc, is dissolved by the action of diluted nitric acid; and by the fear that too large a mass of oxidable metal would be required to produce decisive results. After reflecting, however, for some time on the slow and weak action of sea water on copper, and the small difference which must exist between their electrical powers and knowing that a very feeble chemical action would be destroyed by a very feeble electrical force, I resolved to try some experiments on the subject. I began with an extreme case. I rendered sea water slightly acidulous by sulphuric acid, and plunged into it a polished piece of copper, to which a piece of tin was soldered equal to about one-twentieth of the surface of the copper. Examined after three days the copper remained perfectly clean, whilst the tin was rapidly corroded: no blueness appeared in this liquor; though, in a comparative experiment, when *copper alone* and the same fluid mixture was used, there was a considerable corrosion of the copper, and a distinct blue tint in the liquid.

If one-twentieth part of the surface of tin prevented the action of sea water rendered slightly acidulous by sulphuric acid, I had no doubt that a much smaller quantity would render the action of sea water, which depended only upon the loosely attached oxygene of common air, perfectly null; and on trying $\frac{1}{200}$ part of tin, I found *the effect* of its preventing the corrosion of the copper perfectly decisive.

5. This general result being obtained, I immediately instituted a number of experiments, in most of which I was assisted by Mr. FARADAY, to ascertain all the circumstances connected with the preservation of copper by a more oxidable metal. I found, that whether the tin was placed either in the middle, or at the top, or at the bottom of the sheet of copper, its effects were the same; but, after a week or ten days, it was found that the defensive action of the tin was injured, a coating of sub-muriate having formed, which preserved the tin from the action of the liquid.

With zinc or iron, whether malleable or cast, no such diminution of effect was produced. The zinc occasioned only a white cloud in the sea water, which speedily sunk to the bottom of the vessel in which the experiment was made. The iron occasioned a deep orange precipitate: but after many weeks, not the smallest portion of copper was found in the water; and so far from its surface being corroded, in many parts there was a regeneration of zinc or of iron found upon it.

6. In pursuing these researches, and applying them to every possible form and connection of sheet copper, the results were of the most satisfactory kind. A piece of zinc as large as a pea, or the point of a small iron nail, were found fully adequate to preserve forty or fifty square inches of copper; and this, wherever it was placed, whether at the top, bottom, or in the middle of the sheet of copper, and whether the copper was straight or bent, or made into coils. And where the connection between different pieces of copper was completed by wires, or thin filaments of the fortieth or fiftieth of an inch in diameter, the effect was the same; every side, every surface, every particle of the copper remained bright, whilst the iron or the zinc was slowly corroded.

A piece of thick sheet copper, containing on both sides about sixty square inches, was cut in such a manner as to form seven divisions, connected only by the smallest filaments that could be left, and a mass of zinc, of the fifth of an inch in diameter, was soldered to the upper division. The whole was plunged under sea water; the copper remained perfectly polished. The same experiment was made with iron: and now, after a lapse of a month, in both instances, the copper is as bright as when it was first introduced, whilst similar pieces of copper, undefended, in the same sea water, have undergone considerable corrosion, and produced a large quantity of green deposit in the bottom of the vessel.

A piece of iron nail about an inch long was fastened by a piece of copper wire, nearly a foot long, to a mass of sheet copper, containing about forty square inches, and the whole plunged below the surface of sea water; it was found, after a week, that the copper was defended by the iron in the same manner as if it had been in immediate contact.

A piece of copper and a piece of zinc soldered together at one of their extremities, were made to form an arc in two different vessels of sea water; and the two portions of water were connected together by a small mass of tow moistened in the same water: the effect of the preservation of the copper took place in the same manner as if they had been in the same vessel.

As the ocean may be considered, in its relation to the quantity of copper in a ship, as an infinitely extended conductor, I endeavoured to ascertain whether this circumstance would influence the results; by placing two very fine copper wires, one undefended, the other defended by a particle of zinc, in a very large vessel of sea water, which water might be considered to bear the same relation to so minute a portion of metal as the sea to the metallic sheeting of a ship. The result of this experiment was the same as that of all the others; the defended copper underwent no change; the undefended tarnished, and deposited a green powder.

Small pieces of zinc were soldered to different parts of a large plate of copper, and the whole plunged in sea water: it was found that the copper was preserved in the same manner as if a single piece had been used.

A small piece of zinc was fastened to the top of a plate of polished copper, and a piece of iron of a much larger size was soldered to the bottom, and the combination placed in sea water. Not only was the copper preserved on both sides in the same manner as in the other experiments, but even the iron; and after a fortnight, both the polish of the copper and the iron remained unimpaired.

7. I am continuing these researches, and I shall communicate such of them as are connected with new facts, to the Royal Society.

The Lords Commissioners of the Admiralty, with their usual zeal for promoting the interests of the Navy by the application of science, have given me permission to ascertain the practical value of these results by experiments upon ships of war; and there seems every reason to expect (unless causes should interfere of which our present knowledge gives no indications) that small quantities of zinc, or which is much cheaper, of malleable or cast iron, placed in contact with the copper sheeting of ships, which is all in electrical connection, will entirely prevent its corrosion. And as negative electricity cannot be supposed favourable to animal or vegetable life; and as it occasions the deposition of magnesia, a substance exceedingly noxious to land vegetables, upon the copper surface; and as it must assist in preserving its polish, there is considerable ground for hoping that the same application will keep the bottoms of ships clean, a circumstance of great importance both in trade and naval war.

It will be unnecessary for me to dwell upon the economical results of this discovery, should it be successful in actual practice, or to point out its uses in this great maritime and commercial country.

I might describe other applications of the principle to the preservation of iron, steel, tin, brass, and various useful metals; but I shall reserve this part of the subject for another communication to the Royal Society.

VII. *A finite and exact expression for the Refraction of an Atmosphere nearly resembling that of the Earth.* By THOMAS YOUNG, *M. D. For. Sec. R. S.*

Read February 5, 1824.

IT has lately been demonstrated, in the Journal of the Royal Institution, that if the pressure of the atmosphere ,y, be represented either by the square or by the cube of the square root of the density ,z, the astronomical refraction ,r, may be obtained in a finite equation. Mr. IVORY, in a very ingenious and elaborate paper lately presented to the Royal Society, has computed the refraction, by means of several refined transformations, and with the assistance of converging series, from an equation which expresses the pressure in terms of the density and of its square: I have now to observe, that if we substitute, for the simple density, the cube of its square root, and make $y = \frac{3}{2} z^{\frac{3}{2}} - \frac{1}{2} z^2$, we shall represent the constitution of the most important part of the atmosphere with equal accuracy, although this expression supposes the total height somewhat smaller than the truth, and belongs to one of those hypotheses, which Mr. IVORY has considered as inadmissible: it has the advantage, however, of affording a direct equation for the refraction, which agrees very nearly with Mr. IVORY's table, and still more accurately with the French table, and with that which has been published for some years in the Nautical Almanac.

Since $\mathrm{d}x = \frac{-\mathrm{d}y}{mz}$, m being the number of times that the modulus of the atmospherical elasticity is contained in the radius of the earth, and here $\mathrm{d}y = \frac{9}{4}\sqrt{z}\,\mathrm{d}z - z\,\mathrm{d}z$, we have $\mathrm{d}x = -\frac{9}{4}\frac{\mathrm{d}z}{m\sqrt{z}} + \frac{\mathrm{d}z}{m}$, and $\int \mathrm{d}x = -\frac{9}{2m}\sqrt{z} + \frac{z}{m} + \frac{7}{2m}$, for the height above the earth's surface, which, when $z = 0$, becomes $\frac{7}{2} \times 27300 = 95550$ feet. For the refraction, we have the equation $\mathrm{d}r = \frac{-p\,\mathrm{d}z}{\sqrt{(x^2 - s^2 - 2p\,[1 - z])}}$ (Astr. Coll. XV.) $= \frac{-p\,\mathrm{d}z}{\sqrt{(2\int \mathrm{d}x + v^2 - 2p + 2pz)}}$, which is the value originally assigned to this fluxion by Dr. BROOK TAYLOR; v being the sine of the apparent altitude; and here

$$\mathrm{d}r = \frac{-p\,\mathrm{d}z}{\sqrt{\left(\frac{7}{m} - \frac{9}{m}\sqrt{z} + \frac{2z}{m} + v^2 - 2p + 2pz\right)}};$$

or, if $\sqrt{z} = \psi$, and $\mathrm{d}z = 2\psi\,\mathrm{d}\psi$,

$$-\frac{\mathrm{d}r}{2p} = \frac{\psi\,\mathrm{d}\psi}{\sqrt{\left(\frac{7}{m} - 2p + v^2 - \frac{9}{m}\psi + \left[\frac{2}{m} + 2p\right]\psi^2\right)}}.$$

which is equivalent to the $\frac{x\,\mathrm{d}x}{\sqrt{(a + bx + cxx)}}$ of the Article FLUENTS in the Encyclopædia Britannica, No. 259; the fluent being

$$\frac{1}{c}\left[\sqrt{(a + bx + cx^2)} - \frac{b}{2\sqrt{c}}\,\mathrm{hl}\,(2cx + b + 2\sqrt{c}\sqrt{[a + bx + cx^2]})\right],$$

and its whole value, from $z = 1$ to $z = 0$, being $-\frac{cr}{2p}$ $= \sqrt{(a' + v^2)} - v + \frac{b}{2\sqrt{c}}\,\mathrm{hl}\,\frac{2c + b + 2v\sqrt{c}}{b + 2\sqrt{c}\sqrt{(a' + vv)}}$, putting $a' = \frac{7}{m} - 2p$, since $a + b + c = v^2$.

For the numerical values of the coefficients, taking, at the temperature of 50°, $p = .0002835$, and $\frac{1}{m} = .001294 = \frac{1}{772.8}$, $a = \frac{7}{m} - 2p + v^2 = .008491 + v^2$, $b = \frac{-9}{m} = -.011646$, and $c = \frac{2}{m} + 2p = .003155$; hence $\frac{2p}{c} = .17972$, $\sqrt{a'} = .0921466$, $\sqrt{c} = .05617$, $\frac{-b}{2\sqrt{c}} = .10367$, $2c + b = -.005336$, and

$$r = .17972\left(\sqrt{(.008491 + v^2)} - v - .10367\,\mathrm{hl}\,\frac{.005336 - .11234\,v}{.011646 - .11234\sqrt{(.008491 + vv)}}\right);$$

and at the horizon, when $v = 0$,

$$r = .17972 \left(.0921466 - .10367 \text{ h l } \frac{.005336}{.011646 - .11234 \times .092147} \right) =$$

$.009840 = 33'\,42'',5$; which is only $1'',5$ less than the quantity assigned by the French tables and in the Nautical Almanac, while Mr. Ivory makes it $34'\,17'',5$. Again, if we take $v = .1$, for the altitude $5^\circ\,44'\,21''$, we obtain $8'\,49'',5$ for the refraction, while the Nautical Almanac gives us $8'\,53''$, and Mr. Ivory's table $8'\,49'',6$. There is however no reason for proceeding to compute a new table by this formula, the method employed for the table in the Nautical Almanac being rather more compendious in all common cases; and even if it were desired to represent Mr. Ivory's table by the approximation there employed, we might obtain the same results, with an error never much exceeding a single second, from the equation

$$.00028333 = \frac{v}{s} r + \frac{2.26 + \frac{1}{2} vv}{ss} r^2 + 5400 \frac{rr}{ss} \left(\frac{v}{s} r + \frac{1.13 + \frac{1}{3} vv}{ss} r^2 \right).$$

Welbeck Street,
3rd. February, 1824.

VIII. *The Bakerian Lecture. On certain motions produced in fluid conductors when transmitting the electric current.* By J. F. W. Herschel, *Esq. F. R. S.*

Read February 12, 1824.

1. Having had occasion, in the course of some enquiries respecting the decomposing agency of the Voltaic pile, to electrify mercury in contact with various saline solutions, I was surprised to observe motions take place in the fluid metal of a violent and apparently capricious kind, for which, as I had uniformly operated with very feeble electric powers, there seemed no adequate cause. Frequently it would be agitated with convulsive starts; sometimes currents and eddies of great violence would be formed in it; at others, it would spread and elongate itself, ramifying out into the most irregular forms; and altogether presenting appearances of a nature so singular, as induced me to make experiments with a view to ascertain their cause, or at least the circumstances essential to their reproduction.

2. The singular convulsive agitations into which mercury is thrown when placed within the circuit of a powerful Voltaic battery discharged through water, has been noticed by Sir H. Davy, in his Elements of Chemical Philosophy. Pure water, however, is so very imperfect a conductor, that great Voltaic powers must be used; and the phænomena are then too irregular, and the agitations too violent for distinctness.

It is only when liquids which conduct well are used to form the circuit, that they become regular, and can be studied at leisure under the influence of moderate electric energies.

3. If a quantity of very pure and perfectly clean mercury, free from the slightest superficial film, be placed in a Wedgewood-ware evaporating basin (which must also be scrupulously clean), and covered to the depth of about a quarter of an inch with concentrated sulphuric acid, and the extremities of two wires of platina in connexion with the poles of a Voltaic* apparatus be immersed *in the acid only* on opposite sides of the mercury, but not in contact with it; immediately a rapid circulation will be seen to take place in the acid, owing to a violent current which establishes itself between the two wires, setting directly across the mercury in a direction from the negative (or zinc) towards the positive (or copper) pole. This current is kept up steadily, and without any change in its direction or force so long as the pile remains in activity, and only flags, and at length ceases, when its energy is quite exhausted. The mercury is not sensibly tarnished or otherwise acted on, and, after the experiment, is found to have undergone no change; nor is the acid sensibly altered, with the exception of the trifling portion decomposed, and a minute quantity of mercury taken up.

4. If we examine the phænomena more attentively, we shall observe that the particles of the acid in immediate contact *with the mercury*, are those which move most actively, being

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* The battery I employed in this and the subsequent experiments (unless where the contrary is expressed), consisted of ten pairs of single plates, each of fourteen square inches in surface, excited by mixed nitric and sulphuric acids much diluted.

darted along its surface with surprising violence; those above them, and more remote, appearing rather to be dragged or forced along by them, than impelled by any force acting directly on themselves. We shall perceive too, that, if some distance intervene between the wires and the edges of the mercury, the current will be confined, and the circulation take place in the immediate neighbourhood of the mercury *only*, the liquid around the wires being nearly, or quite at rest.

5. If the centre of the globule or disc of mercury be situated in one straight line with the extremities of the wires, the current will set diametrically across it; but if this be not the case, it will follow a curvilinear course, every elementary filament of it having a different curvature, and each traversing the mercury in a path having a common origin and termination, viz. the points (*z*) and (*c*) of its surface nearest to the negative and positive poles respectively.

6. If the globule of mercury be of considerable size (four hundred or five hundred grains for instance), it will be observed to elongate itself in the direction of its axis towards the negative wire, and if near enough, will reach and amalgamate with it: but if it be small, its whole mass will move bodily with more or less rapidity, as if attracted to the negative wire. This apparent attraction is often very energetic, the globule moving with great velocity towards the negative wire, to which it immediately adheres. If the wires form a triangle with the situation of the globule while at rest, the latter advances neither directly to the negative, nor directly from the positive wire, but in a direction oblique to both, approaching the negative wire in a spiral, and describing frequently several revolutions with increasing velocity before it

ultimately falls into and amalgamates with it, like a body acted on at once by an attractive force tending to the negative, and a repulsive, from the positive wire.

7. These apparent attractions and repulsions, this elongation of large masses of mercury and bodily motion of small ones toward the negative pole, are in reality, however, only secondary effects; their immediate cause, as well as that of the currents in the surrounding acid, will be discovered by a more minute attention to what takes place in the mercury itself, while under the influence of the electric action.

8. To this end, if we operate on a considerable mass of mercury, and, instead of covering it with the acid, merely moisten it and the containing vessel, making the circuit as before, only by the medium of the thin film of acid which adheres, the circulation of the mercury will be not less violent; but it will then be evident that the origin of the motion is in the mercury itself, the acid film being (so far as mechanical impulse is concerned) merely passive, and dragged along by its adherence to the mercury, coating it frequently with a stratum so thin as to reflect iridescent colours over its whole surface, and render the phænomenon extremely beautiful. The motion of the mercury consists in a continual radiation of its superficial molecules from the point nearest to the negative pole, by which it is kept in a constant state of circulation, each particle being urged along the surface from the negative to the positive pole and returning along the axis. Were the mercury insulated from contact with the bottom of the sustaining vessel, and devoid of adhesion to the liquid, the momentum of the portions going and

returning would be equal, and the centre of gravity of the whole mass would remain at rest; but by reason of the friction and adhesion of the fluid metal to the vessel and liquid, these re-act on the globule in a direction contrary to that of the superficial currents, and the centre of gravity accordingly advances in that direction, or towards the negative pole. When this motion cannot take place, the internal current, having all one uniform direction, forces its way outwards to the negative pole, distorting and elongating the figure of the mercury in proportion to its energy. If the metal be oxidated, so as to give a certain tenacity to the superficial film, the radiating currents pursue their course under it; and the supernatant fluid, being thus defended from their action, remains at rest. In this case the only indication of their existence is the protuberance produced by the resultant interior streams.

9. A number of singular appearances are explained by this internal current. In some cases the mercury throws out projections or probosces of inordinate length, which take the direction of the electrified wire, and follow all its motions. The resultant interior current is in this case directed along the axis of the proboscis from its root to its extremity, which thus becomes an indication of a very powerful radiation along its surface in an opposite direction. In others, the mercury flattens throughout its whole extent, and, when this is the case, it is always covered with a thick coat of oxide. In these circumstances the superficial currents tend from the circumference towards the centre of the flattened mass, and the interior stream tends from the centre outwards in all directions,

in a horizontal plane, thus continually urging the circumference farther and farther out, by diminishing the radius of curvature of the vertical section of its edge.

10. That friction against the vessel is the principal cause of the apparent attraction of a globule of mercury to the negative end, may be proved evidently by the substitution of a glass for a Wedgwood-ware basin. In this case the currents are produced as before; but, though equally forcible, the globule shows little or no tendency to move bodily, but if placed on a plate of emeried glass, or on any other rough surface, it will move with great activity; nay, so strong is its tendency to the negative pole, that globules of considerable magnitude may thus be sustained without contact of either wire, on surfaces many degrees inclined to the horizon.

11. It is essential to the production of the motions in question, that the mercury be in actual contact and free communication with the acid, and so situated as to be within the influence of the electric current. It is not necessary, however, that a continuity of the acid should subsist between the positive and negative wires; they will appear in any interrupted circuit of mercury and the liquid medium. The experiment indeed is difficult to try in sulphuric acid, whose capillary attraction for mercury is such that the least drop, applied to any part of a clean surface of that metal, instantly spreads over the whole, but with other conducting media it may readily be made. We have only to drop a little of the liquid to be tried on two different spots of a large clean surface of mercury, and bring the poles in contact with them, taking care not to plunge them in the metal, when the same phænomena will be observed to take place about each pole as

if the whole surface had been covered with the liquid. The motions however are confined to such portions of the mercury as are actually covered, all the rest remaining quite still: the effects too are modified by capillary action.

12. When the circuit is completed in a conducting liquid, in the manner described in the beginning of this paper, the action is most forcible in the direct line joining the poles; its violence diminishing as we recede from this line, though it continues sensible to a great distance either way: and the course pursued by electricity in its passage through conducting media, and its law of distribution within it, may in some degree be traced, by placing globules of mercury in different parts of a liquid; when it will be plainly seen, that it is by no means confined, or nearly so, to the straight line between the poles, or to the surface of the conducting medium, but immediately on quitting the wires diffuses itself through the whole liquid, its density being a maximum in the space directly between them, and diminishing rapidly as we recede from their line of junction.

13. The mechanical action appears (*cæteris paribus*) to be proportional to the absolute quantity of electricity which passes, *dato tempore*, through a filament of the liquid at the point where it is exerted. The magnetic effect is proportional (*cæteris paribus*) to the absolute quantity of electricity in motion present at once, (or at any indivisible instant of time) in a given portion of the conducting wire, or within the sphere of action of the needle, that is, to its density.* To establish or

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* In these expressions I have conceived electricity as being transmitted through conductors according to the laws of a gas of high, but variable elasticity through pipes more or less obstructed, a supposition which will represent many of the

refute this distinction, will require experiments which it is easy to imagine, but which I have not yet had an opportunity of making. At first sight, indeed, the phænomena in question present a considerable analogy to the electro-magnetic vortices observed in the fluid metals; but on presenting very powerful magnets to the mercury, while under the circumstances above described, in various positions, I have never been able to perceive any influence exerted by them in accelerating, retarding, or deviating the currents; and moreover, these are incomparably more forcible in proportion to the electric powers used, than the motions produced by the action of magnets.

14. In consequence of this superior energy of action, the phænomena which form the subject of this Paper, furnish a test, perhaps, the most sensible yet known of the developement of feeble Voltaic powers. I constructed a small battery of zinc and copper wires twisted together, each pair being two inches long from the point of junction, and the wires $\frac{1}{30}$ of an inch thick. Ten pairs of these, excited by extremely dilute nitric acid, caused a rapid rotation in mercury, interposed under sulphuric acid between the poles, and a regular advance of

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phænomena. The sluggish electricity of a single pair of plates may be compared to air, rendered dense and less elastic by excessive cold, while the active charge of a powerful battery, or the spark of an ordinary electrical machine, is in this view assimilated to air with all its energies exalted, and its density diminished by violent heat. The same quantity in weight may pass through the same conducting pipe in the same time; but in the one case the motion of each molecule will be comparatively much slower, and the actual quantity present at any instant of the discharge (*e. g.* an inch in length) of the conductor, much greater than in the other. I am well aware that this is merely an analogical representation of facts, and of course inaccurate, but it serves to explain the distinction in the text.

globules of that metal towards the negative pole. The rotation continued with considerable force, when the wires were so far withdrawn as to have only their extremities in contact with the liquid in the cells, in which case the surface exposed by each pair to the action of the acid could not exceed $\frac{1}{50}$ of a square inch. Nay, so delicate is this indication, that the electricity developed by bringing the extremities of a thin zinc and copper wire in contact with a glass merely moistened with the above mentioned dilute acid, is abundantly sufficient to cause an immediate and unequivocal rotation in an ounce or two of mercury properly exposed to its action. By this means, indeed, the feeblest electrical excitement may be placed in evidence. I have thus rendered *strikingly* sensible the electricity developed by a mere difference in the state of the surface of two small portions of copper wire from the same coil (one being a little cleaner than the other) not above an inch in length of either being immersed; or that set in motion by a copper and zinc wire held near together and dipped in common pump water, powers which it is not easy to render sensible by other means. For the success of these experiments, however, it is not enough merely to plunge the extremities of the conducting wires under sulphuric acid. The surfaces of contact *here* require to be greatly increased,* so as to insure the transmission of the whole of the electricity developed. The best way is to immerse them in two considerable pools of mercury under the acid, one on either side of the globule to be set in rotation.

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* The efficacy of an increase of surface for transmitting electricity *into* a liquid, is remarkable. By bringing the positive pole in contact with a large surface of mercury, or still better, of an amalgam of mercury and zinc, over which a saline solution is poured, the reduction of the metals of the alkalies and earths at the other pole is operated with a degree of facility hardly to be imagined without trial. In this way the decomposition of ammonia may be effected with thtee pair of single plates of the above dimensions, in very moderate action.

15. Hitherto we have considered only the effect produced when a current of electricity is transmitted over mercury through sulphuric acid. When other conducting liquids and other metallic bodies are used, phænomena of the same kind are produced, but so modified by the nature of the substances employed, the intensity of the electric power, and the manner of conducting the experiments, as to become extremely perplexing; and I must warn the reader who may be inclined to repeat them, that he must expect to find them frequently fail, or even give contrary results from those I shall describe, owing to causes by no means easy to discover. The principal is impurity in the mercury used, and none should be used but what has been carefully distilled, and well washed with dilute nitric acid. It was long before I discovered this necessity; and ignorance of this essential condition engaged me in a series of tedious and disheartening repetitions of every experiment, till I was on the point of relinquishing the subject in despair, encountering contradictory results in operations conducted, as I then supposed, in a manner precisely similar.

16. When mercury, so purified and perfectly clean, is placed in any conducting liquid, and the circuit completed without bringing either pole in contact with the metal, the phænomena vary with the nature of the liquid; but, generally speaking, the effect is the production of currents more or less violent, radiating from the point nearest the negative pole.

In the acids, particularly in the more powerful and concentrated ones, and such as are good conductors of electricity, they are decided and violent. In saline solutions their force is less, in proportion as the electro-positive energy of the base is greater. Thus, in the salts with a basis of potash they are feeble, and often only perceptible by a momentary start of the mercury when the circuit is completed. In those of soda, ammonia, baryta, strontia, and lime, they are more distinct, while in salts of magnesia, alumina, and the metallic oxides, their influence is still more sensible. On the other hand, under solutions of the pure alkalies and alkaline earths, the mercury remains quite quiescent, or at most is only agitated by feeble and irregular motions, depending on causes not now in contemplation.

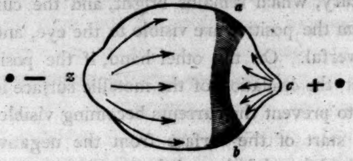
17. In many liquids, and especially in solutions of the nitrates, there is formed not only a current radiating from the negative pole, but also one from the positive, which even has in some cases a preponderance over the other. These co-exist in the mercury; and, in consequence of their action, a zone of equilibrium is formed in the globule, nearer to one or the other pole, as the antagonist current is more or less violent. The best way to render the influence of this counter-current sensible, is to operate on a large quantity of mercury, under dilute solutions, keeping the negative pole at a distance, and the positive very near. In this way there are few liquids which, when the pile is in good action, do not show some signs of a counter-current from the positive pole. The cause of this will be evident, when we come to speak of the action of metallic alloys.

18. If either pole be brought in contact with the mercury, no

currents are observed from the point of contact (at least when the mercury is fresh and the contact perfect) but strong ones are always produced, radiating from the other. If it be the negative pole which is made to touch, it amalgamates with the mercury, which remains bright, and the currents radiating from the positive are visible to the eye, and generally very powerful. On the other hand, if the positive pole be in contact, the oxidation of the metallic surface is usually so rapid as to prevent the currents becoming visible, but a momentary start of the surface from the negative wire, the flattening of the globule, and the protuberances it throws out in pursuit of the oppositely electrified conductor, sufficiently indicate their existence under the crust of oxide. Where this oxidation however does not happen, or is prevented by the addition of a few drops of dilute nitric acid, the currents from the negative wire are equally evident with those from the positive, just mentioned.

19. These however are not the only effects produced by contact with the electrified wires. On breaking the contacts and completing the circuit in the liquid, the mercury is found, for the most part, to have acquired new properties, or lost some of its former ones. A globule of four or five hundred grains of pure mercury being introduced into a solution of sulphate of soda, the circuit was completed in the liquid with neither pole in contact. A current was produced from the negative pole. A momentary contact being made with that wire, and the circuit then completed as before in the liquid, a counter-current was produced from the positive pole, more confined in the sphere of its extent, but apparently more violent in its action than that from the negative. In conse-

quence, the globule acquired the figure here annexed, having a blunt elongation at z, the point nearest the negative pole, and a more pointed one at c, that next the positive, with a kind of shoulder at ab. The film of oxide produced at z was thus swept towards c, but never attained beyond the zone ab, where it remained stationary and constant in quantity, being absorbed at the side next c as fast as it was produced at the other. Another short contact was now made with the negative wire, and, on breaking it, the currents from c were found to have increased both in strength and extent, while those from z were proportionally enfeebled, the zone of equilibrium ab being thus brought nearer to z. By another contact prolonged a few seconds, the negative currents were contracted within a very small space around z, and by prolonging the contact a little longer, its influence was totally destroyed, and a regular and violent circulation from + to — established throughout the whole globule.



20. But the effects did not stop here. On prolonging the contact a considerable time, the negative current (from z) was not only wholly destroyed, but changed into one of a contrary tendency; *i. e.* radiating in all directions *to* z; the particles of the mercury appearing to be attracted to that point with a force equal, or superior, to that with which they were

repelled from *c*. The positive pole being held at some distance, and the negative directly over the surface, any scum or impurity on the mercury was observed to collect directly under it, in a small circular spot, following exactly its motions; and when this was cleared away, the fluid metal was violently thrown up towards the wire in a jet of two or three tenths of an inch in height.

21. The mercury was now brought into contact with the positive wire. Visible oxidation did not commence on its surface for a long while, during which time violent currents still continued to radiate in all directions from the wire and *towards* the point *z* (or in a direction opposite to what they would have taken in untouched mercury). By degrees, however, a counter-radiation commenced opposite to the negative pole, whose sphere was at first very limited, but gradually extended, producing a zone of equilibrium, which advanced rapidly towards the positive wire, and at length attained it. The instant this took place, the oxidation of the mercury commenced at *z*, and speedily extended over the whole surface, forming a thick crust.

22. If the contact of the positive pole was continued long enough, the mercury, on cleansing it from its coat, was found reduced to its former state, as if freshly introduced; but if broken as soon as the crust was fully formed, a radiation from the negative wire was produced, and the crust broken up and swept by it to *c*, where it collected, and was hurried off. But the moment this was done, and the surface of the mercury had become bright throughout, it stopped for an instant, and immediately a violent revulsion took place and a powerful current radiated from *c*, that from *z* being annihilated.

23. These effects, when first observed (not connectedly in regular succession, as here set down, but piece-meal), appeared exceedingly perplexing; but the key to them was soon found. I observed that the effect of a contact of the negative pole was proportionally stronger in producing a positive radiation, as the mercury had been allowed to circulate longer before the contact was made, and, on more close examination, I found that the platina wire terminating the negative conductor of the pile, had got amalgamated with a little mercury, which, during the time the circuit was completed in the liquid, had become alloyed with sodium; and, with the quantity of this metal judged to be present, the effect seemed always to be in proportion. I had no hesitation, therefore, in attributing all the new properties acquired by the mercury to the presence of sodium, and on introducing into a quantity of the pure metal a small quantity of an amalgam of this substance prepared for the purpose, I found my supposition verified; a most violent negative rotation being immediately produced on completing the circuit, without allowing either wire to touch the mercury.

24. The presence of this highly electro-positive metal therefore counteracts the effect of the negative pole, and exalts that of the positive in a degree proportioned to its quantity, till at length it completely overcomes, and even reverses the former effect. As the quantity (in the foregoing experiment) diminished in the alloy by the oxidating action of the positive pole, the mercury, as we have seen, by degrees resumed its original properties. The only effect that may appear obscure, is the revulsion noticed in the direction of the currents when the last portion of oxide disappears. It is, in fact, a pretty com-

plicated effect, but capable of easy explanation. The oxidation takes place over the surface of the metal before the last portions of sodium are removed. This is easily proved. We have only to break the circuit altogether, and the crust of oxide will gradually disappear (unless suffered to go too far), being reduced by the sodium beneath it. Were it then not for the crust of oxide, the currents, as has been seen, would be in a positive direction. But the oxide, acting on the stratum of metallic molecules immediately below it, deprives them of their alloy, which it converts into alkali, leaving a stratum of pure mercury. Now we have seen that in *this*, the rotation, in the circumstances of the experiment, would have a negative direction. We have only then to admit that the peculiar action by which the rotations are caused, is confined to the common surface of the mercury and liquid, to have a perfect idea of the mode in which the whole process is carried on. The stratum of pure mercury on the surface is removed by a negative current agreeably with its natural relations, and immediately succeeded by a stratum of the sodiuretted metal from the interior; this, in its turn, is deprived of its sodium by the oxide in contact with it, and is immediately radiated off like its predecessor, and so on till the whole crust of oxide is exhausted or swept off, when the remaining mercury, still retaining an excess of sodium, and instantly rendered homogeneous, is acted on as an alloy in the way already described.

25. That sodium is actually present in the mercury when it has acquired the property of producing currents from the positive pole, (which for brevity I will hereafter call the positive property) by contact with the negative wire, may be shown by

a very simple and interesting experiment. When the negative wire is detached and the circuit broken, the mercury lies quiet at the bottom of the vessel, with the exception of a slight irregular motion on its surface, and now and then a minute gas bubble disengaged. Now touch it under the liquid with a clean metallic wire of any kind (provided its extremity be not allayed with sodium), and a violent action instantly commences. The mercury rushes on all sides to the wire in a superficial current as if to give out its sodium, while a copious stream of hydrogen is given off from the wire, not merely at the point of contact with the mercury, but wherever it touches the liquid. In a word, the sodium, the wire, and the liquid form a voltaic combination, and the electricity produced by the contact is sufficiently powerful to decompose the aqueous portion of the latter in great abundance. The action lasts for a longer or shorter time accordingly as the mercury is more or less highly charged with the alkaline metal, rarely, however, for more than 10 or 12 seconds, and when over, the mercury is found to have lost its positive property, and to be reduced to its pristine state, (provided the contact be made with copper or platina), which a long immersion in the fluid without such contact would not have entirely effected.

26. If the mercury thus charged with the alkaline base be not entirely covered with the fluid, and the metallic contact be made at the vertex of the globule, out of the liquid, no effect is produced; but if the other end of the metallic wire be bent round and brought to touch the liquid at some distance from the mercury, the violent action above described immediately commences; with this difference, that *now* the surface

of the mercury is radiated in all directions *from* the point of contact to the circumference of the globule, and that the whole of the hydrogen is given off at the other end of the wire where it touches the liquid. A little consideration will suffice, however, to show that both these effects are merely modifications of one and the same. It is not to, or from the *wire* as such, that the superficial particles radiate; they merely follow the direction of the predominant electric currents in their passage *through the liquid*. It is in fact the case of the source of positive electricity, being the mercury itself, instead of its being conveyed to it from a pile at a distance.

27. Having thus distinctly traced the alteration in the mechanical effect by contact with the negative pole, to the amalgamation of the mercury with sodium, the knowledge of this fact led me to investigate more minutely the effects of different metals in their contact and amalgamation with mercury; and the results I have encountered in the course of these enquiries, appear to me so remarkable, that I cannot forbear annexing them, especially as they afford an explanation of almost every anomaly which perplexed me in the commencement of the investigation. In order to render the effects less liable to objection, as well as more distinct and striking, I now used solutions of potash or soda, pretty highly impregnated with the caustic alkali, for the conducting liquid. This has the advantages at once of high conducting power, and of producing no currents whatever in pure mercury, neither pole being placed in contact. Of course, whatever motions arise on the introduction of an extraneous metal must be due entirely to the presence of that metal, and the mercury may

be regarded as merely passive, so far at least as mechanical action is concerned.

28. *Potassium.* A contact of a single second's continuance with the negative pole of a pile of eight pairs, in feeble action under liquid potash, imparted to 100 grains of mercury the property of rotating violently from the positive to the negative pole, the circuit being completed in the liquid alone. The rotation was forcible when this alloy was diluted with 100 grains more of pure mercury, and was still sensible after the addition of another equal quantity. In this latter case, the quantity of potassium present could hardly be estimated at a millionth part of the whole mass.

29. *Sodium.* Under a solution of soda I electrised 100 grains of mercury during 80 seconds with the above mentioned Voltaic power, the mercury being in contact with the negative wire. It was then washed hastily, and introduced under a glass bell into dilute muriatic acid, which disengaged 95 mercury grain measures of pure hydrogen. Consequently, it contained less than $\frac{1}{50}$ of a grain of sodium; and as in such extremely small quantities the production of the alloying metal must go on uniformly, a contact of 1″ would have produced only $\frac{1}{80}$ of the quantity, or $\frac{1}{4000}$ of a grain; that is $\frac{1}{400,000}$ of the whole mass. This being premised, a contact of 1 second in duration was made under similar circumstances, with 100 grains of fresh mercury, which was thus found to have acquired a powerful rotatory property. This was now diluted with 100 grains more of the pure metal, in which, therefore, the sodium was only in the proportion of 1 to 800,000. The rotation was enfeebled, but was still full and distinct. Being again diluted with 100 grains more of mercury, so as to make the propor-

tion of sodium 1 : 1,200,000, there was still a considerable radiation from the positive pole, but not extending over the whole surface. On reducing the proportion of sodium by a third addition of an equal quantity of the pure metal to 1 : 1,600,000, a feeble radiation was still sensible in the same direction.

30. *Ammonium.* A considerable quantity of the amalgam of this singular substance introduced into mercury under a solution of soda *did not communicate to it any power of rotation.* This remarkable result, which goes to separate ammonium by a definite character from the other metallic bases of the alkalies, was again obtained on repeating the experiment. It is possible, indeed, that a complete insolubility of the amalgam in pure mercury may be the cause of this want of action, but the supposition must be allowed to be a very forced one.

31. *Barium.* This metallic body amalgamates with the utmost readiness with a power of eight pairs of plates when the muriate is acted on; a small globule of mercury at the negative wire throwing out beautiful arborescences, and fixing into a highly crystalline, pretty permanent, solid amalgam. A very minute quantity of this introduced into mercury under solution of soda, gives it the positive property. Its efficacy, in reversing the direction of the currents, is strikingly sensible when introduced into a quantity of mercury kept in a state of negative rotation under oxalic acid. The amalgam of mercury and barium added in small quantities to pure mercury, imparts to it the same property as we noticed in the case of sodium, of forming a Voltaic combination with a wire brought in contact with it under a saline solution, and the action so produced is much more lasting.

32. *Strontium, Calcium.* These metals, in my experiments with the feeble powers used, manifested a remarkable indisposition to alloy with mercury. The small quantity of calcium deposited on an amalgamated negative wire obstructed its contact with a larger globule of mercury to such a degree, that no electric communication could be established. Under a solution of strontia, the contact of the negative wire imparted the positive rotatory property sensibly, though very feebly. That this was not merely owing to the low conducting power of the liquid, was proved by introducing a minute quantity of the amalgam of zinc, when the mercury immediately commenced rotating strongly. The influence of *magnesium* is more sensible than that of strontium or calcium, from the greater readiness with which it amalgamates.

33. *Zinc.* When pure mercury is electrified under solutions of potash or soda, with neither pole in contact, in the manner so often alluded to, it shows no signs of rotation, as has already been observed; but, if touched for an instant with the end of a clean zinc wire, or if an atom of the solid amalgam of zinc, the smallest that can be taken up on the end of a needle, be added to it, it instantly rotates violently in a positive direction (or from the positive pole).

34. An alloy of one part zinc to 10,000 of pure mercury rotates with the utmost violence. When this is diluted with ten times its quantity of the latter metal, the force of rotation appears but little impaired. The proportion of mercury was increased to 400,000 : 1, and the rotation, though feeble, was yet complete, pervading the whole of a considerable mass of the alloy; and even when the zinc amounted to no more than a 700,000*th* of the whole, a current radiating to a short dis-

tance from the positive pole was still sensible: when, however, the zinc formed only a millionth part, no difference could be perceived between the alloy and pure mercury.

35. *Lead.* An alloy of 200 parts of mercury and 1 of lead possessed the positive property in perfection. When the proportion of mercury was 667 to 1, the rotation was still produced, but was not full and regular. When increased to 1000, a slight, but sensible current, was perceived to radiate from the positive pole to a short distance; but a proportion of 2000 mercury to 1 lead extinguished every trace of motion.

36. *Tin* acts also in the same way, and with nearly the same energy, as far as I could judge by the eye. It is certainly much inferior to zinc.

37. *Iron* communicates the property in question, though present in such minute quantity as not to be detected by prussiate of potash. On the other hand,* *Copper* does not communicate it, though its proportion be increased to such a degree as to give a blue solution in nitric acid, and even to render the mercury quite sluggish.*

38. Of the other metals I have tried, *Antimony* is the only one which appears to exert a perceptible action, and this is so slight (never amounting to more than a mere start, or slight convulsion of the surface at the first impression) that I am inclined to attribute it to impurities in the antimony used, especially as this metal stands very low in the scale of electro-

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* The amalgam of iron obtained in one experiment was a white friable solid of a lustre between silver and iron: the mercury being driven off by heat, the iron took fire, and glowed like a live coal till reduced to the state of black oxide, soluble in muriatic acid, having all its characters.

positive energy. Bismuth, silver, and gold, though present in considerable quantities in the mercury, impart to it no power of rotation whatever.

39. This property then of the metals, bears an evident relation to their electro-positive energies. It even affords something like a numerical estimate of them; rude indeed, and liable to a thousand objections, but still not without its value in our present state of complete ignorance on that most interesting of all chemical problems. If it be true, that the whole of chemistry depends on electrical attractions and repulsions, every thing which offers a prospect, however remote, of one day arriving at an exact knowledge of the intensities of these forces, must be regarded as of consequence. It may be objected, that it is only the excess of the electro-positive energy of the alloying metal over that of the mercury, or the alloy over the liquid, that we measure in these experiments, by the quantity of it required to impart a certain appreciable momentum. Yet it is something to have rendered it probable, that this excess in the cases of sodium, zinc, and lead, are in proportions not *very* remote from 1,600,000; 700,000; and 1000; or 1600, 700, and 1. The effect being purely mechanical, even the intensity of the motive forces exerted on a molecule of one of these metals could be determined, did we know the law of its action—but at least, in our ignorance of this, we are sure that it must be incomparably superior to gravity. A mass of mercury an inch in diameter alloyed with $\frac{1}{100,000}$ its weight of zinc, revolved with a motion so rapid as to complete the transfer of particles floating in the liquid in less than a second across its surface. Now, even if we were to take the supposition of a uniform acceleration of the

motion of a molecule from one end to the other of this transfer, the intensity of gravity being taken at unity, that of the force accelerating each particle of the alloy would amount to $\frac{1\text{ inch}}{16\text{ feet}\times(1'')^2} = \frac{1}{12\times16} = 0.00521$, and each particle of zinc being loaded with 100,000 times its weight of inert matter, the intensity of the force, acting on its molecules, cannot possibly be so little as 521 times their gravity. But it is in all probability immensely greater. So far from being uniformly accelerated along their whole course, the molecules, if narrowly watched, will be evidently seen to move with less and less velocity as they recede from their point of radiation; and it is assuming little to suppose their velocity at a hundredth of an inch from this point double of their mean velocity with which they traverse the diameter. To produce this effect, the force must (if supposed to act uniformly through *this* small space) be increased 100 fold, or to an intensity upwards of 50,000 times that of gravity. Such considerations tend, if I mistake not, greatly to enlarge our views of nature, and to prepare us for the admission of the most extravagant *numerical* conclusions respecting bodies less within the reach of our senses. That such minute proportions of extraneous matter should be found capable of communicating sensible mechanical motions, and properties of a definite character, to the body they are mixed with, is perhaps the most extraordinary fact that has yet appeared in chemistry. When we see energies so intense exerted by the ordinary forms of matter, we may very reasonably ask, what evidence we have for the imponderability of any of those powerful agents to which so large a part of the activity of material bodies seems to be owing?

40. I was anxious to examine whether similar motions would be produced in other metals than mercury and its alloys, when in fusion. The foregoing experiments, indeed, leave little room to doubt their capability to do so; but the nature of the case throws great difficulties in the way of direct experiment. I have been successful hitherto only in the case of the fusible alloy of lead, tin, and bismuth, no mercury being present. This, with a little management, may be preserved tolerably clean of film and air bubbles, when kept in fusion under a boiling solution of sugar, acidulated with phosphoric acid, in which case the same circulation takes place as in the case of mercury, viz. from the negative to the positive pole. When solution of sugar alone however was used, the influence of the tin and lead became sensible, the predominant radiation being from the positive pole; a feeble counter-current being, however, observed from the negative.

41. The contact of the positive pole, in like manner, communicates peculiar properties to mercury, but less strongly marked, and which appear to depend, in part, on the film of oxide formed on its surface, and partly on an absorption of oxygen by the metal itself; a thing rendered not improbable by the analogy of silver and other metals, which when fused in contact with air, absorb oxygen without losing their metallic appearance. The facts I have observed are chiefly these:

42. Equal quantities of mercury were electrified for equal times in two separate capsules, under similar solutions of carbonate of soda, one in contact with the negative wire, and the other with the positive. On mixing them together, the mercury was acted on as if pure, and showed no signs of

containing sodium. Here, the mercury in contact with the positive pole had acquired a virtue capable of counteracting the effect of a considerable impregnation of sodium, which, had it not been counteracted, could not fail to be violent.

43. When mercury is kept in contact with the positive pole, the surface contracts a film of oxide of more or less considerable thickness. Now, break not only the contact, but the circuit. The mercury will be quite still; but the moment it is touched with a clean metallic wire (not electrified), the oxide disappears rapidly at the point of contact, as if absorbed, and the remainder rushes in on all sides to supply its place, producing a system of current in the surface radiating towards the wire. It is not indifferent with what metal the contact is made; potassium, sodium, barium, tin, and zinc, are those which produce the most violent action, the surface brightening instantly with a kind of flash like the *brandishing* of melted silver, tin being in this respect superior to zinc. The effect of iron is pretty considerable, that of copper less so, and of antimony and platina, none at all; neither had phosphorus any effect.

44. The effect, therefore, depends on the oxidability and amalgamating property jointly; and this points out the *modus operandi.* An amalgamation takes place at the point of contact, and this brings the oxidable metal into chemical contact with the oxide immediately around that point, which is instantly reduced. The motion of the surface is, however, doubtless an electric effect, for when mercury, *not* recently electrified is touched, under acids, &c. with metallic wires, the effects are not the same. The contact of copper, for instance, produces an immediate, and even strong radiating

current *from* the point of contact instead of *to* it, and this ceases the moment the contact becomes perfect by amalgamation, and cannot be renewed but by cutting off the amalgamated end, and making a fresh contact.

45. When mercury is electrified in contact with the positive pole under *certain* metallic solutions (nitrate of copper for instance), and the circuit broken, removing both wires, the current continues feebly for some time after the electric power is withdrawn, in the same direction (viz. from the point (z) opposite to the negative pole. By degrees, it grows more forcible, and a film formed during the electrisation is swept along to the point (c) opposite the former position of the positive wire, where it accumulates, leaving at length, the portion of the surface at z quite bright As soon as this happens, the currents increase considerably in strength, and radiate with great violence from the point z. This spontaneous action continues often for a long while. If the negative pole be made to act in succession, opposite to two points z, z', of the mercury, and be then quickly withdrawn and the circuit broken, both these points become centres, from which spontaneous currents radiate simultaneously in all directions. If the negative pole be made to act vertically over a large flat surface, when the circuit is broken, a violent spontaneous radiation emanates from the point immediately below the place where it was situated.

46. If the wires be only withdrawn so as to complete the circuit in the liquid, the film formed during the contact of the positive pole is swept to the point c, opposite that pole; and a violent current is established, radiating from z to c. If this be suffered to continue some time, and the circuit be then

broken, the motion continues as if the electricity still passed; but if the mercury be agitated, so as to break the crust collected at *c*, the regularity of the motion is disturbed: the surface of the mercury is thrown into a kind of fritillation, owing to an immense number of minute and very rapid vortices; and it is not till after some time that a regular and uniform direction of the currents is re-established.

47. These phenomena demonstrate the existence of a system of currents radiating *towards* every molecule of the crust on the surface. In consequence of this, so long as the latter is broken up into small portions and distributed over the whole surface, the currents are irregular and undecided; but as soon as these portions begin to be swept together and collected, they assume a uniform direction, viz. towards that part where, from contact of the vessel or other cause, they meet with no counter currents to oppose them. In what manner the crust acts is however still a little obscure: in all probability it forms a Voltaic combination with the mercury and the liquid.

48. In reasoning upon the facts detailed in this Paper, we have to consider, as probably materially influencing the results, first, the vast difference of conducting power between the metallic bodies set in motion, and the liquid under which they are immersed. This is not unlikely to enter as one of the essential conditions of the phænomenon, especially as it appears to result from all the experiments, that the peculiar action, whatever it be, by which the currents are produced, is exerted only at the common surface of the fluids. I have never been able to produce the least trace of such currents without the presence of a fluid metal. This leads us to conclude that a second essential condition is a perfect immiscibi-

lity of the conducting fluids, so as to render the transition from one to the other quite sudden. Besides these, a third essential condition is to be found in a certain chemical, or electrical relation between them. Under these conditions, it is by no means impossible, that the phænomena may admit of complete explanation from what we already know of the passage of electricity through conductors, and the high attractive and repulsive powers of the positive and negative electricities *inter se*. It is very possible, for instance, that a highly electro-positive body, as potassium, present in the mercury, may have its natural electric state exalted by its vicinity to the positive pole; and, being thus repelled, may take the only course the resistance of the metal on the one hand, and attraction of cohesion on the other, will permit; viz. along the surface, to recede from the positive pole. It *may* even act as a carrier of positive electricity, which *may* adhere to it too strongly to be transmitted through the mercury (which, though a good, is far from a perfect conductor;) and when arrived at the opposite side of the globule, may there, by the influence of the opposite pole, lose its exalted electrical state. This explanation tallies with that of other phænomena which have been attributed to a similar cause; I mean the tendencies observed in the vapours of electro-positive and electro-negative bodies to conductors electrified oppositely, which Mr. Brande has described in a Bakerian Lecture formerly read to this Society. Yet it must not be concealed that this explanation is beset with difficulties, and that the mode of action of the less-conducting medium in it is far from clear; it does not even appear why such a medium is at all necessary, unless we conceive it to retard, or otherwise modify the electric current, in its passage through it, and dispose it thereby to ready combination with the metallic molecules.

49. Another course is doubtless open to us, which is to consider the action which takes place at the common surface of two unequally conducting media, as one, *sui generis*, and to depend on a new power of the electric current of a nature, bearing some analogy to the magnetic action, or possibly resulting from it; but this in the present state of our investigation would be too bold an hypothesis, especially as it is also a very vague one.

50. But whatever conclusions we may form, the phænomena are certainly interesting, and promise to afford abundant matter for future research. Meanwhile, it is not improbable that many phænomena of minute intestine motions usually attributed to capillary attraction, generation of heat, or other causes, may be referable to similar causes. One I cannot forbear to mention, from the striking *external* resemblance of the effect to some of those described in this Paper. I mean the motions described by M. Amici in the sap of the chara, as originating in certain rows of globules disposed in the direction of the stream. The motion of the fluid in the vicinity of these globules has been attributed by M. Amici himself to electricity developed in some unknown manner by them, and is so similar to what takes place when a stream of electricity is made to pass over a row of minute globules of mercury under a conducting medium, that one has difficulty not to presume an analogy in the causes.

J. F. W. HERSCHEL.

Slough, January 6, 1824.

NOTE.

51. Since writing the above, Mr. FARADAY has been so good as to show me a Paper, published by M. SERRULAS, in the Journal de Physique for 1821 (vol. 93,) in which are related one or two of the appearances described in this Lecture, and other very curious ones referable to the same causes (though not apparently regarded by him as being so.) As the phænomena themselves are interesting, and the theory of them adopted by him is (as I shall easily show) insufficient, I shall be pardoned for extracting the whole passage from his Memoir; regretting at the same time not having been able to find a former Paper on the subject, mentioned by him, in which his explanation is given at full length.

52. The phænomena in question relate to the singular gyratory motions assumed by alloys of potassium when floated in small fragments on mercury under water. After noticing those of the alloy of bismuth, which he describes as particularly forcible and lasting, he goes on to say,

53. " Ne seroit-il pas intéressant d'étudier l'action électrique qui se manifeste dans cette circonstance pendant l'oxidation du potassium."—" Elle me semble digne d'attention pour sa liaison avec la décomposition de l'eau dont elle depend uniquement. * * * *

54. " La pellicule légère qui se forme dans ce cas n'est que le bismuth divisé provenant de l'alliage retenant entre ses molecules des bulles d'hydrogène extrêmement fines. Cette pellicule, comme je l'ai dit, est attirée avec une grand promptitude par les substances métalliques mises en contact avec le

mercure sur lequel les fragmens d'alliage sont en mouvement.

55. " J'ai du considérer cette pellicule comme jouissant de l'électricité positive, attendu qu'elle se porte vivement vers l'extrémité negative d'une cuve en activité, et qu'elle est au contraire puissamment repoussée par le pole positif. Si les deux conducteurs touchent seulement l'eau du bain, l'attraction et la repulsion ont lieu dans le sens indiqué. L'effet est encore le même si l'un des fils touche le mercure, et l'autre l'eau. La pellicule se fixe au pole negatif d'où elle est chassée avec force par l'approche du pole opposé. Elle s'écarte, et l'hydrogène de l'eau décomposée se dégage sur ses bords qui dans ce cas font partie du conducteur et le terminent. Si les deux fils plongent dans le mercure il est bien entendu qu'il ne se manifeste plus rien.

56. " Quand, au lieu d'eau simple, le bain de mercure est couvert d'une dissolution peu chargée du chlorure de sodium, le tournoiement des fragmens est plus lent. L'hydrogène produit se trouve engagé et retenu presqu'entièrement par la pellicule du bismuth ; l'eau en devient nebuleuse. A l'instant où l'on a plongé dans le bain une tige métallique, on remarque autour de celle-ci un frémissement ; les mouvemens cessent et sont arrêtés tant que la tige reste plongée ; elle fixe la pellicule dans toute l'étendue du bain ; les fragmens d'alliage y sont emprisonnés ; mais aussitôt que la tige est retirée, *l'effluve d'hydrogène écarte la pellicule, et les mouvemens recommencent.*

57. " Un fil plongé sur un point quelconque d'un bain ou tournoie l'alliage, même dans un endroit éloigné de ce tournoiement, la partie plongée de ce fil se couvre en peu de

temps d'une multitude des bulles d'hydrogène. Ne pourroit-on pas encore d'après cette observation, *qui prouve que toute la surface du bain est parcourue d'hydrogène*, ne pourroit-on pas trouver dans l'émission rapide et abondante de ce gas la cause de l'électricité, quand on considère que l'air atmosphérique dirigé avec une soufflet sur un carreau de verre donne a ce carreau l'électricité vitrée; ou bien cette effluve d'hydrogène qui pousse vivement sur le mercure les molecules de bismuth non amalgamé, qui les réunit sous forme de pellicule, produit entre les deux métaux un frottement qui développe cette électricité."

58. From these passages it seems natural to collect, that M. SERRULAS conceives, 1st, the production, motion, &c. of the pellicle on the surface to originate in the actual mechanical impulse of streams of hydrogenous matter (*effluve d'hydrogene*,) radiated in all directions from the potassium in the moment of its oxidation. That, 2ndly, this bodily radiation of hydrogen is propagated along the surface to any distance. That, 3rdly, the hydrogen disengaged in bubbles from a metallic wire plunged into the mercury is this actual radiant hydrogen, conveyed and collected on its surface from all parts of the mercury. That, 4thly, the friction of the hydrogen so radiated produces the electricity, and not the electricity the hydrogen. And, lastly, that the gyration of the fragments themselves is a consequence of the re-action of the hydrogen they dart out during their oxidation by the water.

59. All these phænomena, however, are much better accounted for on the principles of this Lecture, from a knowledge of the properties conferred on mercury by alloying it with potassium; but, first, it is necessary to premise, that the mere

contact of a metal capable of amalgamating, even for an instant, communicates its peculiar properties, almost in the moment of contact, to the whole mass. The experiments in Art. 33, abundantly prove this; and it may be readily shown also by the following. Let a quantity of mercury be placed in a vessel of muriatic acid; no action takes place; but if touched with a zinc wire it presently becomes covered with bubbles, copiously disengaged from every part of the surface.

60. In the circumstances of M. SERRULAS's experiments, it is therefore obvious that his mercury must have been always sensibly impregnated with potassium and the supernatant liquid, a solution of potash; and that it was so, is proved by the effects of the electric current, which agree precisely with those I have stated, as being always produced in such circumstances (Articles 18, 28;) but the cause assigned to these effects by Mr. S. viz. the electro-positive energy *of the pellicle*, is proved not to be the real one by the simple fact, that the violence of the motion is always proportional to the cleanliness of the surface, and is greatest when there is no pellicle at all; besides which, the pellicle *here* consisted of metallic bismuth, a substance incapable of producing any such effect as shown in Art. 38.

61. The gyration of the fragments is produced as follows: a strong Voltaic excitement takes place at the point of contact of two metals so different as mercury and potassium. The mercury becomes strongly positive, and the floating fragments negative. The circuit is completed by the alkaline liquid; and the mercury, being alloyed with a portion of potassium, and being itself the positive pole of the combination, we have here the case of Art. 21; and the result, as stated

by M. SERRULAS, is precisely as in that experiment, the currents radiating from the point of immersion. These once produced, drive before them the fragment in which they originate, in the direction in which it exposes the greatest surface to their action.

62. The attraction of the pellicle to a metallic rod plunged into the mercury, is also a direct consequence of the alloy of potassium present in the mercury, as is also the disengagement of gas from the wire. It is, in fact, precisely the experiment described in Art. 25, and has nothing whatever to do either with the floating fragments, or with any hydrogen they may be discharging at the time, farther than that their contact serves to furnish potassium to the mercury.

63. It is needless, therefore, to push this examination farther, as all the phænomena observed by Mr. S. are only particular cases of those I have described. With regard to the radiant hydrogen producing currents by its impulse, I would ask how it happens that currents are produced (when the positive pole is placed in contact), while a thick and tough coat of oxide covers the whole surface; and, one would think, must effectually defend it from the action of the hydrogen. Yet we have seen, in Art. 18, that the currents continue their course under this crust; and it will hardly be contended, that the hydrogen finds a passage between the oxide and the metal.

J F. W. H.

London, Jan. 13, 1824.

IX. *Experiments and observations on the developement of magnetical properties in steel and iron by percussion:—Part II. By* William Scoresby, *Jun. F. R. S. E. &c. Communicated by Sir* Humphry Davy, *Bart. Pres. R. S.*

Read January 29, 1824.

Having had the honour of laying before the Royal Society a Paper on the "Developement of Magnetical Properties in Steel and Iron by percussion,"* I beg permission to add to that Communication an account of other experiments; in which much higher degrees of magnetic energy were obtained by percussion, in the employment of new combinations of rods of iron not previously magnetic.

It was shown in the former paper, that the extraordinary developement of magnetism, by this process, arose from the use of a large bar of iron, or soft steel, first rendered magnetic by hammering, in the position of the dipping needle, or in the direction of the magnetic force; for, on applying a similar quantity of percussion to the same bar whilst held on a mass of brass or stone, or even on a bar of iron laid in the plane of the magnetic equator, the polarity elicited did not exceed the twenty-ninth part of that obtained by the use of the vertical rod of iron.

In the subsequent experiments I had two principal objects in view. First, to try, by a new combination of auxiliary rods of iron, to attain still higher degrees of magnetic energy: and, Secondly, to endeavour to ascertain on what circumstances, as to the magnitude of the rods of iron, and the quality, size, and temper of the steel wires, the highest success of the experiment depends.

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* Philosophical Transactions for 1822, p. 241.

In the experiments formerly detailed, a single rod of iron was used, and the steel bars or wires were hammered upon it, whilst both were held in a vertical position; in which case the magnetism of the iron, after hammering, was employed in aid of the power of percussion for the developement of the magnetism of the steel bars. But the magnetism of the iron rods was communicated only to the lower, or *north* end of the steel bars, the polarity of the upper, or *south* end, being merely consequential. Hence it appeared probable, that, were the steel bars or wires placed between two rods of iron, and thus subjected, either directly, or indirectly, through the medium of the upper rod, to percussion, they would derive the advantage of the magnetism of both rods of iron acting consistently, and at the same time, upon their northern and southern poles, so as, I apprehended, by the use of equal rods of iron, to *double* the quantity of attraction formerly developed in the steel.

As, however, two long rods of iron were not so easily managed as a long one for below and a shorter for above, I prepared my rods on this plan, though with the expectation of sacrificing a proportion of power.

The rods I ordered for the experiment were of the respective lengths of three feet and one foot, and an inch in diameter. The weight of the former was 8lbs. and of the latter $2\frac{2}{3}$lbs. The end of the larger rod (I i*), designed to be kept upward, was made conical, with the view of concentrating its magnetic force; but this was truncated at the diameter of a quarter of an inch, and a shallow hole drilled in the centre for steadying the steel wires when hammered, the lower ends of which wires were rounded into a blunt point, so as to fit the depression in the top of the iron rod. The lower end of the shorter rod (I ii), was constructed in a similar manner.

As I could not hammer this conical extremity of the large iron rod without destroying its form and face, I made use of another iron rod (I iii), corresponding in size and weight with the rod (I ii), but having its lower extremity hollowed like a cup, to fit upon the conical termination of the larger rod (I i.) The larger rod was generally hammered before each experiment through the medium of this shorter rod, (I iii), which not only served to preserve the other rod from injury, but at the same time tended to augment its magnetism.

In experiments with this apparatus, then, after eliciting some magnetic energy in the rod I i, by hammering it in the way just described, the steel wire intended for receiving the magnetism was placed between the two rods, with the conical

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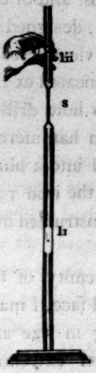
* To prevent circumlocution, I have distinguished the rods and wires used in the following experiments by the initial letters descriptive of the substance of which they were composed, connected with a number to distinguish the different bars and wires of the same quality. Thus I, signifies an iron rod or bar; and S, a steel bar or wire; S *u*, steel untempered, or in the state in which the wire was drawn; and S *t*, steel tempered, or softened by heating to redness, and slowly cooled.



terminations [I i and I ii], the whole three substances forming a continuous straight line, in a vertical position, as in the annexed figure. The upper end of the upper rod (I ii), being now beaten with a small hammer,* acquired magnetical properties, which were communicated to the steel wire, whilst the lower rod receiving some influence from the percussion, performed a similar office. This kind of experiment I have, in the subsequent details, denominated the *compound process;* whilst the hammering of a wire upon the principal rod (I i), only, without the use of the second rod, is called the *simple process.*

To give the experiments the best chance, I made use entirely of steel wire, such as is employed by watch-makers, which I found, from several comparisons and experiments, that I did not think it necessary to detail here, had a much higher capacity for magnetism thus developed, than any other steel that I tried: and for obtaining the best effect, in the trial of the lifting power produced in the steel wires by percussion, I procured a series of nails of different weights, made of good iron, with flat heads, so as to stand with their points upward, in which position they are the most readily lifted; and after blunting their points, I gave them some degree of polish for improving the contact.

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* The hammer employed in all the subsequent experiments weighed eleven ounces, inclusive of the shaft. Some trials were made with a larger hammer; but its tendency to bend the wires was so great, as more than to counterbalance the advantage it gave of a more speedy developement of magnetic energy. This larger hammer, however, (weighing twenty-five ounces), was generally used for beating the iron rods before the different experiments, when it was of decided advantage.

These nails were of the following weights in grains:— $1\frac{1}{2}$, 4, $5\frac{3}{4}$, $7\frac{1}{2}$, 18, 37, 73, 88, 112, 186, 246, 326, 389, and 482.

In trials of the lifting power of the steel wires, in the subsequent experiments, the weight of the largest nail that could be lifted is set down.* In some instances a small weight was added to the nail, when the difference between this and the next in the series was considerable; in this case, the lifting power is stated at the weight of the nail, *plus* the number of grains of the appended weight.

FIRST SERIES,

for determining the superiority of the Compound process over the Simple process, when employed for the developement of magnetism in steel wires by percussion; and for the trial of the general effect of Temper on the magnetic attraction elicited.

The apparatus consisted of the iron rods I i, I ii, and I iii, together with various steel wires.

Experiment No. I.

[July 1.]

Steel wire S u i, [namely, steel wire untempered]. Length, 5 inches; diameter, $\frac{1}{8}$th of an inch; weight, 167 grains.

This wire was hammered, for a length of time, on the *simple process*, and only lifted 36 grains.

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* I am perfectly aware that the lifting power of a magnet is by no means a certain measure of its magnetic force, and that the comparative lifting powers of magnets of various sizes do not afford an exact measure of their relative degrees of magnetic energy; but this mode of trial was employed both on account of the impossibility of making accurate observations on magnetic intensities at sea, where

The hammering being continued for some time longer, by the *compound process*, the lifting power was sensibly augmented; but it yet refused 73 grains.

No. II.

The same steel wire (S t i) softened by heating to redness.

a. By simple process.

After 5 blows this wire now lifted 73 grains.
4 more blows - - - 186.

The wire being now bent was straightened, by which it lost a great part of its magnetism; it was therefore raised to a lifting power of 186 grains, as before, by a number of very slight blows.

10 gentle blows were now struck, but its lifting power was still - - - - 186 grains.
20 more blows increased its attractive force very sensibly; but it still refused the nail of 246 grains.

These last 30 blows having been productive of very little effect, the power was considered as being sufficiently near the maximum by this process, to serve as a comparison with the compound process.

b. By Compound process.

20 slight blows on the upper iron rod, I ii, now increased the lifting power of the wire to 246 grains.
And 20 more slight blows raised its power to 326.
But 60 more slight blows produced no higher effect.

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these experiments were made, and also on account of this being the most palpable and striking test of the high force of attraction attained by the process.

No. III.

[July 2.]

Same wire as in the last experiment [S *t* i].

a. Simple process After repeatedly hammering this wire upon the bar I i, it now lifted 246 grains; but its power could not be farther increased.

b. Compound process. The lifting power, by often hammering, was now raised to 326 + 19 = 345 grains.

No. IV.

[July 10.]

Wire S *t* i, *the same as in the last experiment.*

The compound process was, on this occasion, continued for a length of time, the bars I i and I iii being repeatedly hammered together, but no additional lifting power could be obtained.

No. V.

[July 10.]

Larger wire, S *u* i i; *length,* 12 *inches; diameter,* $\frac{1}{8}$*th of an inch.*

This wire being hammered in its untempered state, with 20 or 30 smart blows, by the simple process, only lifted $7\frac{1}{2}$ grains.

No. VI.

The same wire as in the last experiment, tempered by heating to redness. [S *t* i i.]

a. Simple process.

By 2 smart blows, with a small hammer, it
now lifted - - - - - 37 grains.

4 more very strong blows	73 grains.
10 more	186
8 more	246
6 more [wire began to bend]	326
4 more	326

No. VII.

The wire last used (S *t* ii), *not being regularly softened, was again put through the fire, and all its magnetism destroyed.*

a. Simple process.

By 2 strong blows only, it was now made to lift 186 grains.

4 more	246
30 lighter blows	326
20 more [began to bend]	326

b. Compound process.

After 20 blows on I ii, (the magnetism before given not being destroyed, though much weakened by straightening it) it lifted 326 grains much more freely than before, but refused the next larger nail of 389 grains.

No. VIII.

A piece of the same wire as that used in the last experiment, 2½ *inches in length.* [S *t* iii.]

a. Simple process.

After being repeatedly hammered by this process, its lifting power was raised to 56 grains.

Hammered several times, at intervals, for a minute or two together, without increasing its power to lift 88 grains.

b. *Compound process.*

After 12 smart blows, under this process, the lifting power was augmented to 186 grains.

SECOND SERIES,

for ascertaining more exactly the comparative effects of a difference of temper on wires of the same quality and dimensions, and of the relative advantages of the different processes for the developement of magnetism by percussion in equal wires.

The apparatus consisted of the three iron rods (I i, I ii, and I iii,) with five new wires, from the same piece, of equal sizes, each of them being five inches in length, and 145 grains in weight; namely, S *u* iv, S *t* iv, S *t* v, S *t* vi, and S *t* vii.

EXPERIMENT No. IX.

New steel wire, S *u* iv *(untempered); not in the least degree magnetic.*

a. *Simple process.*

5 smart blows struck on this wire whilst held on bar I i, (which bar had previously been struck 20 hard blows with the large hammer, through the medium of the bar I iii) occasioned a lifting power of 37 grains.

b. *Compound process.*

[Magnetism of the wire destroyed.]

5 smart blows on this process, (each of the bars I i and I ii having been previously hammered whilst held vertically in the hand) produced a lifting power of 37 + 18 = 55 grains, and very nearly 73 grains.

No. X.

New Steel wire S *t* iv *(softened), not at all magnetic.*

5 hard blows struck on this wire whilst held on the equator, or middle of the bar I i, (this bar lying horizontally instead of standing vertically as in the other experiments) produced only a lifting power of 4 grains

No. XI.

Similar wire to the last, S *t* v, *not in the least magnetic.*

5 blows struck on this wire, whilst held upon I i, both the bar and wire being in a vertical position, and both being freed from magnetism immediately before the experiment excepting the magnetism of position, occasioned a lifting power of 37 grains, and that with difficulty.

No. XII.

A wire similar to the last in every respect, S *t* vi ; *not in the least degree magnetic.*

a. Simple process.

5 blows were struck on this wire, held in the same position as in the last experiment on I i, the bar I i having been previously struck 20 hard blows with the large hammer, through the medium of I iii, by which treatment a lifting power was given to the wire of 112 grains. The magnetism of the wire being destroyed again, the same experiment was repeated, and somewhat harder blows struck, by which, with 5 blows, a lifting power of 186 grains was obtained.

No. XIII.

Another wire exactly similar to the last in size, quality, and temper.

b. Compound process.

5 blows on this process, through the medium of I ii, (both the bars I i and I ii having been previously hammered to the same extent as before) produced a lifting power of 246 + 19 = 265 grains.

THIRD SERIES,

for determining the effect of larger iron bars.

In place of the rod I i, *a large bar of iron* (I iv) *was substituted. Its length was* 8 *feet, and its diameter* 1¾ *inches. The rest of the apparatus (rods* I ii *and* I iii*) was the same as before.*

No. XIV.

[July 14, &c.]

Wire S*t* i *being* 5 *inches in length,* ⅛*th in diameter, and* 164 *grains in weight,* which, with the former apparatus, was made to lift* 345 *grains.* [*See Experiments* II. III. *and* IV.

a. Simple process.

Though the magnetism of this wire was nearly destroyed before this experiment, a few smart blows on I iv, gave it a ready lifting power of 326 grains.

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* The weight of this wire was originally 167 grains; but it was reduced 3 grains by occasionally filing the end.

The experiment being repeated the next day, (July 15), when the bar, I iv, had become more magnetic by use, the wire readily lifted - - - - 339 grains.

A few more blows raised its power to - 482

Additional hammering - - 482 + 19 = 501

Experiment repeated, July 16, 482 + 51 = 533

b. Compound process.

After a few smart blows the lifting power of this wire was now raised to - - - - 482 + 103 = 585.

Process continued by a number of blows 482 + 122 = 604.

After a variety of repetitions of this process, the large bar, I iv, having become strongly magnetic, the lifting power of the wire was augmented to 482 + 187 = 669 grains, being above *four* times its own weight.

No. XV.

[July 14 to 18]

A new untempered wire (S *u* iii), *from the same piece as* S *t* i, $4\frac{1}{2}$ *inches in length; weight,* 150 *grains.*

a. Simple process.

After a few smart blows, this wire lifted - 246 grains.

On repeated hammerings, its lifting power was augmented to - - - - - 326

The process repeated the next day, - - 389

Again repeated on another day - - 482

b. Compound process.

On the first application of this process, by a few hard blows, there was no accession of power, the lifting power continuing at - - - - - 482 grains.

But on repeating the process several times on the following day, the power rose to 482 + 120 = 602.

This process was again tried some days afterwards, and applied with considerable labour, the hammering being continued for half an hour together, sometimes by one process and sometimes by the other. At the commencement of the operations it was found to have retained of its former energy a lifting power of 389 grains. This was considerably augmented, but it never reached quite so high as before.

No. XVI.

Wire S t ii, 12 inches in length and one-sixth of an inch in diameter; being the same that was used in experiments V, VI. *and* VII. *when the maximum lifting power obtained was between 326 and 389 grains.*

a. b. Both processes alternately.

After a little hammering this wire now lifted 389 grains.

But after many repetitions on the same day [July 15] it refused 482 grains, though its former power was somewhat increased.

a. Simple process.

Some days after the preceding trial, when the great bar, I iv. had become strongly magnetic (after the conclusion of experiment No. XIV), this long wire was again subjected to experiment, when its lifting power was increased to 482 + 180 = 662 grains.

A piece of this wire, four inches in length, was now cut off, and the larger part was thrice hammered by the simple process, when its lifting power was 482 + 90 = 572, being a loss of 90 grains.

GENERAL REMARKS.

The results of the foregoing experiments were, in general, satisfactory, though there were some trifling anomalies, which will be noticed in their order. The chief points I had in view were tolerably well determined; but the investigation is so far from being complete, that what has been done, as is often the case in such researches, opens a much wider field of enquiry than I at first contemplated. I proceed, however, to state the chief results obtained, consisting of such deductions as I trust the experiments will be considered legitimately to warrant.

1. One principal object of enquiry in these experiments was, to prove the effect of a combination of rods of iron, on a plan previously arranged, for augmenting the magnetic power by percussion. The result was, in the main, perfectly agreeable to my expectations. For, by the employment of such a combination, which I have denominated the *compound process*, the magnetism developed was always more or less increased, but the proportion of augmentation was by no means regular, nor is the law by which it is governed very obvious. In experiment No. II, the maximum effect of the simple process was an attractive force capable of lifting between 186 and 246 grains; whilst the compound process readily augmented the lifting power to 326 grains. And on repeating the trial, (Experiment III.) on the simple process, the wire obtained a lifting power of 246 grains, which the compound process increased to 345. But the advantage was the most obvious in experiment No. VIII, when a very short piece of wire was used. In this case, whilst the simple pro-

cess only occasioned an attractive power capable of lifting between 56 and 88 grains, though the wire was repeatedly hammered, the compound process augmented the lifting power, by 12 blows, only, up to 186 grains, indicating three times the former magnetic energy.

But, on the other hand, in experiment No. VII, the application of the compound process to a very long wire, was productive of very little advantage. Nor was the compound process of so evident an advantage in the third series of experiments: in this case, however, the failure of effect was probably occasioned by the very small size of the upper rod, I ii, in comparison of the magnitude and mass of the lower.

2. Another object of enquiry in these investigations was the effect or relation of *temper*, in connection with the degree of magnetic energy developed. The result with the first apparatus, was conformable to the law of magnetics in general; namely, that the *softer* the temper the more susceptible the steel becomes of the magnetic condition. By a comparison of experiments No. I. with II., V. with VI., and IX. with XII. and XIII., the advantage of softening the wire is very obvious. In the first instance, a steel wire in the state in which it was drawn could only be made to lift between 36 and 73 grains; but the same wire, on being softened, readily lifted 186 grains, after nine blows on the simple process, and after 80 blows by both processes, its lifting power was augmented to 326 grains, being nearly twice its own weight, and at least five times the power it acquired in its untempered state. But this difference of susceptibility for receiving the magnetic energy, is rendered still more obvious by a comparison of the second set of experiments (No. V. and VI.) A long wire, untempered, being struck 20 or 30

smart blows by the simple process, only acquired a lifting power of between 7½ and 18 grains; whilst the same wire, after being softened and subjected to a similar treatment, lifted 326 grains And in the third set of experiments (IX, XII, and XIII,) the result was analogous. The untempered wire, by 5 blows on the simple process, lifted only 37 grains, and by the same number of blows on the compound process, 73; whilst a similar wire tempered lifted, after exactly the same treatment, 186 grains by the simple process, and 265 by the compound process.

The same result was indeed always obtained in various other experiments, not included in the preceding details, yet there is one in the third series [No. XV.] which is apparently at variance with this conclusion. The difference of effect, however, (the untempered wire in this instance having received an equal power to that of the softened wire of a similar kind), was evidently owing to the employment of a very powerful apparatus, the large bar of which had become highly magnetic from long continued use in these experiments. Hence a greater action upon bars of harder temper was to be expected, conformable to what occurs with the use of a powerful apparatus in the ordinary modes of giving magnetism to steel.

The facility with which magnetism may be developed in softened wires by this process is very striking. The first five blows, in experiment No. II, by the simple process, produced a lifting power of 73 grains, nearly one-half the weight of the wire;—the first two blows, in experiment No. VII, also by the simple process, produced, in a long wire whose magnetism had been totally destroyed by heating to redness, a lifting power of 186 grains; and the first 5 blows, in experiment No. XIII, on a new wire by the compound process, occasioned in this wire, of only 145 grains weight, a lifting power of 265 grains.

3. The increased magnetic energy developed by the use of larger bars of iron (a fact indeed which it was reasonable to expect) is quite satisfactory in all the comparative experiments. The long wire S *t* ii, by the *small* apparatus was made to lift between 326 and 389 grains, being the highest effect produced on it [Experiment No. VII.]; but on applying it to the *large* apparatus, its lifting power was at length augmented to 662 grains. [Experiment XVI.] And by the small apparatus, the highest power that could be given to the wire S *t* i, was a capability of lifting a weight of 345 grains [Experiments II, III, IV,]; but on using the large apparatus, the same wire [Experiment XIV.] was eventually made to lift 669 grains, being above *four* times its own weight. This was the highest effect produced. The advantage, however, of the large apparatus, was the most striking in the case of untempered wires. An untempered wire, S *u* i, had its lifting power, by the first apparatus, with difficulty raised to nearly 70 grains, [Experiment I.] whilst a similar, but somewhat shorter wire, S *u* iii, was, by the use of the large apparatus, readily made to lift 246 grains; and by continuing the process on several different days, its power was at length increased to 602 grains. [Experiment XV.]

4. It would appear from experiments IV. and XV, that in the use of any one apparatus there is a limit to the power which it is capable of developing; whereas, from the great augmentation of effect obtained by the use of a larger apparatus, it becomes probable, that were iron rods sufficiently

large employed, there would be no limit to the attractive force developed in the steel wires, until they were magnetised to saturation. The quantity of effect produced by an iron bar seems to be in some certain proportion to the amount of its own magnetic energy, as indicated by its action on a compass needle, but not in the proportion of its lifting power.

5. It is a well known fact in magnetics, that the capacity of steel for magnetism is increased by time—by repeated renewals of the magnetising process at intervals—and by keeping the magnet under constraint, either by the contact of other magnetic substances, or by the use of conductors between the opposite poles. Hence I expected, that a wire magnetised by percussion to a maximum for the time, might have its power subsequently increased, day after day, in consequence of its capacity being increased by a repetition of the process. To a certain extent this was the case; but when the iron bars had acquired their maximum energy, and the wires were then hammered until there was a decided suspension of increase of energy, no future repetition of the process, however laboriously conducted, gave me any additional power. [Experiments No. IV. and XV.]

On the first view of the subject, I was at a loss to account for the suspension of the augmentation, when I had calculated on an increased capacity for magnetic energy, with the continuation of the process, agreeable to the known laws of magnetics; but I eventually perceived that the analogy, with the ordinary communication of magnetism, was not complete. It was evident that the suspension of augmentation did not arise from the want of capacity in the *steel* for magnetism, the wires not being near a state of saturation; but it arose from the incapability of the *iron rods* attaining, or retaining, more magnetic energy. The maximum, in their conditions, it would appear, determines the maximum in the condition of the steel. But I apprehend, had I used very large rods, capable of communicating magnetism to the wires *above* the point of their saturation, then the usual analogy would have been complete, and I should, progressively, have attained higher and higher degrees of polarity.

As the phænomenon of the progressive elevation of the point of saturation in magnets became a matter of some thought and consideration with me, I may be excused, perhaps, for including here the substance of my reflections on this subject, and especially as some change, similar to what takes place in magnets having their saturated point raised by renewals of the magnetising operation, &c., also takes place in the disposition of steel bars or wires for polarity, by percussion. For, as a magnet having its poles reversed, will not, at first, by the same apparatus and process as that from which it originally derived its power, acquire the same intensity as before; so, in wires magnetised by percussion, with the same end always held downward, the inversion of the poles will not be accompanied by equal power as before. Respecting this phænomenon, I may suggest the following explanation.

The natural condition of iron is without polarity. It acquires polarity by certain modes of treatment, or juxta-position with magnetic substances; until which it evinces no attractive property. In this state its magnetic properties are neutral, and there is no tendency in the iron to develope polarity. But if it be rendered magnetic, no matter by what cause, as it is now in a state of constraint or violence, there is a tendency to return to the state of neutrality. In soft iron, the return to its former condition, is almost instantaneous on the removal of the disturbing cause; but in steel, the restoration to its former state is resisted by a force proportionate to the hardness of its temper. Soft steel readily acquires polarity, but it is evanescent; and hard steel receives polarity with difficulty, but it is very permanent. Though in all magnetised substances there is a force always acting towards the restoration of the condition of neutrality, yet, when the magnetism has been highly developed, it produces a permanent change in the point where the force ceases to act. This point we may call the state of *quiescence*, being the state of a magnetic substance, when the different magnetisms, though not perfectly neutralised, are yet, as it were, balanced. In the natural condition of a substance, capable of some permanency of magnetism, the neutral and quiescent states are coincident, but after every magnetising operation, if the denomination of the poles be always preserved the same, they are farther and farther removed; hence arises an increasing capacity for magnetic energy. If, for the sake of illustration, a piece of steel somewhat soft be magnetised: on the removal of the cause (provided no artificial means be used to keep it up), the polarity will gradually diminish until it comes to the quiescent condition, or the state where the resistance natural to the metal has no returning force to oppose. Suppose, in this state, it yet has polarity capable of lifting the weight of an ounce of iron, and that in its highest or saturated state of magnetism it lifted 20 ounces. On being again magnetised by the same means and process, it will now have increased its capacity to near 21 ounces. By thus continuing the process, and using means to retain the magnetism developed, the quiescent point may perhaps be raised as high as the original state of saturation; that is, to a lifting power of 20 ounces, when its saturated lifting power will approach to 40 ounces. But to effect this, it will be necessary to continue the poles always the same; for on every inversion of the poles, the point of quiescence will return somewhat towards the point of neutrality, and the magnetic energy will be proportionably diminished. On the first change of the poles, the magnetic energy, in a state of saturation, will be diminished by about the difference of the points of quiescence and neutrality, or nearly as much as in experiments with the same poles always preserved, the magnetic energy is augmented. Thus, when in the above example, the point of quiescence has a lifting power of 5 ounces, capable, when magnetised to saturation, of being raised to 25 ounces, on inversion of the poles it will only have a capacity for 15 ounces. These, most probably, are not the exact powers, for it is likely that the augmentation of force, which takes place on repeatedly magnetising at certain intervals beyond the point of quiescence, may diminish in a geometrical ratio, as the point of quiescence recedes from the point of neutrality; otherwise there would be no limit whatever to the power of a magnet. The law, however, not having been determined, I have used in the above examples an arithmetical ratio, merely for the sake of illustration.

This doctrine is the same when applied to the developement of magnetism by percussion, as by any other process, and easily explains why, on the first trial, especially with a weak apparatus, or with bars not perfectly softened, such great effects should not be produced as by numerous repetitions of the same process at different times. The proof as to the cause being the removal of the point of quiescence from that of neutrality, is, I think, quite conclusive. For in an experiment, (one among many to the same effect), made with the wire S *t* i, I found that when 5 blows with a hammer, on the compound process, with the north end of the wire downward (commencing when the attractive force was almost totally destroyed), gave it a lifting power of 326 grains; the same nnmber of blows, with the south end down, (also commencing when the attractive force was very nearly neutralised), gave it only a lifting power of 2 grains. In short, I have raised the point of quiescence so high above the point of neutrality, that a long and severe hammering, south end of the wire downward, only destroyed the original attractive force, but did not produce inversion of poles.

6. The foundation of this process for the developement of magnetism in steel, is the employment of such magnetism of large rods of iron as can be derived from juxta-position of the great magnet, the earth. And the high effects produced seem to depend on the disposition which percussion gives to the ferruginous particles for assuming that condition to which we give the name of *magnetic*. The iron rod employed, as soon as placed in a vertical position, or in the direction of the dipping needle, acquires polarity from the earth; but the natural resistance of its particles to the state of constraint which it must be in when magnetic, prevents its receiving the full power that the earth is disposed to communicate. Percussion, by producing a vibration among the particles of

the metal, seems to overcome this resistance; and, at the same time, disposes the iron for the retention of the magnetism acquired. Now, on placing a wire or bar of soft steel upon this rod of iron, its magnetic virtue occasions some developement of the same power in the steel, which is increased by the tendency that percussion has to aid its developement in the steel, the same as it previously elicited in the iron. By continuing this treatment, the polarity of the steel is progressively augmented until it has acquired a maximum, depending, not so much on its own capacity for magnetism, as on the capability of the iron to develope it. But this condition, it has been shown, is only a maximum so long as the same iron bar or bars are used, and so long as their magnetic energy obtains no augmentation; for if by any means their polarity be increased, the attractive force of the steel wires will rise in proportion. From the whole phænomena viewed in connection, it seems, that the simple general fact is this:—that percussion applied to magnetisable substances in contact with one another, disposes them to an *equality of condition.*

If this view of the subject be correct, we have a satisfactory explanation of some of the phænomena, which were otherwise obscure. We see why large bars, or wires of steel, (though they acquire a greater quantity of magnetic energy, as shown by their higher action on a compass needle, than small ones) do not attain any higher lifting powers than much smaller wires. [Compare Experiments III. with VII., and XIV. with XVI.] And, above all, we have an explanation of the apparently contradictory propositions—that percussion diminishes, and has a tendency to destroy the energy of magnets; and that percussion has a tendency to develope polarity in iron and steel not previously magnetic. Both these propositions are undoubtedly true; but they are under different conditions. Percussion, whilst it diminishes the energy of a magnet when held alone, or even upon an iron bar, would probably augment its energy if it were placed with consistent poles in contact with a more powerful magnet, which is the condition of wires magnetised by percussion; because, if they be hammered upon a substance not magnetic, little or no energy is developed. [See Experiment No. X.] These different effects would appear to be the necessary result of a tendency to equality of condition—a tendency which is precisely similar in respect to bodies of unequal temperatures when placed in contact, or even in juxta-position. For as a hot body placed near, or among, cold substances of greater mass and capacity than itself, has its temperature brought down nearly to the state of the colder substances, whilst a cold body has its temperature raised by contact with hot substances; in like manner, a strong magnet hammered upon an iron rod, has its energy brought down towards the condition of the iron; and a bar not magnetic, or slightly so, hammered upon the same rod, has polarity developed in it, and raised progressively up to the condition of the iron.

As I apprehend it would be in vain to attempt to produce such strong magnetic effects, as we derive from percussion, by the simple use of the same bars and apparatus in any of the known modes of *touching*, it becomes probable that this process might be applied, in connection with other modes of magnetising, for giving increased power to magnets. The extraordinary power of Knight's magnets are well known; though the secret of their manufacture died with him, and no effect at all comparable, has, I believe, been since produced. Is it not probable that he may have employed a process of this description ?*

W. S. *Jun*r.

On board the ship Baffin, off Iceland,
August 27, 1823.

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* I am indebted for this thought to a suggestion of Sir H. Davy's, in a conversation on these experiments.

X. *On Semi-decussation of the Optic Nerves.* By WILLIAM HYDE WOLLASTON, *M.D. V.P.R.S.*

Read February 19, 1824.

WHETHER we consider the astonishing subtlety of that medium, which renders visible to us objects existing at the most immeasurable distances from us, or that delicately constituted organ which, by its general structure, collects the rays of light, and by a nice adaptation of its parts concentrates their force on the sentient fibres of the retina, expanded over its inner surface, we can feel no surprise that such great talents should have been devoted to investigate the curious properties of the one, or that the structure of the other should have been examined with so much assiduity.

The keenness of inquiry manifested by the cultivators of anatomy in observing the most minute parts that have escaped the notice of their predecessors, shows that any addition to the common stock of our information on this subject will be gratifying to a certain portion of the members of this Society, and probably not uninteresting to the Society at large.

It is not my object, in the present paper, to examine either the *first* effect of the cornea in rendering the rays of light convergent, or the power of the crystalline lens in *finally* bringing them to a focus on the retina. It is not my intention to investigate whether the adaptation of the eye to different distances is effected by alteration of the *form* of the lens from its own muscular structure, or by alteration of its *place*, from the agency of other muscles. Nor do I mean to consider either the *involuntary* motions of the iris dependent on the quantity of light present, or that *voluntary* contraction of it by which we adapt the aperture of the pupil for distinct vision at different distances, limiting thereby, what in optics is termed the spherical aberration of the lens.

The subject of my inquiry relates solely to the course by which impressions from images perfectly formed are conveyed to the sensorium, and to that structure and distribution of the optic nerves on which the communication of these impressions depends.

Without pretending to detect by manual dexterity as an anatomist, the very delicate conformation of the nerves of vision, I have been led, by the casual observation of a few instances of diseased vision, to draw some inferences respecting the texture of that part which has been called the decussation of the optic nerves, upon which I feel myself warranted to speak with some confidence.

It is well known that in the human brain these nerves, after passing forwards to a short distance from their origin in the thalami nervorum opticorum, unite together, and are, to appearance, completely incorporated; and that from this point of union proceed two nerves, one to the right, the other to the left eye.

The term decussation was applied to this united portion, under the supposition that, though the fibres do intermix, they still continue onward in their original direction, and that those from the right side cross over wholly to supply the left eye, while the right eye is supplied entirely from fibres arising from the left thalamus.

In this opinion, anatomists have felt themselves confirmed by the result of their examination of other animals, and especially that of several species of fish, in which it is distinctly seen that the nerves do actually cross each other as a pair of separate cords, lying in contact at their crossing, but without any intermixture of their fibres.

In these cases it is most indisputably true, that the eye upon the right side of the animal does receive its optic nerve from the left side of the brain, while that of the left eye comes from the right side; but it is not a just inference to suppose the same continuity preserved in other animals, where such complete separation of the entire nerves is not found.

On the contrary, I not only see reason, from a species of blindness which has happened to myself more than once, to conclude, that a different distribution of nerves takes place in us, but I think my opinion supported by this evident difference of structure in fishes.

It is now more than twenty years since I was first affected with the peculiar state of vision, to which I allude, in consequence of violent exercise I had taken for two or three hours before. I suddenly found that I could see but half the face of a man whom I met; and it was the same with respect to every object I looked at. In attempting to read the name JOHNSON, over a door, I saw only SON; the commencement of the name being wholly obliterated to my view. In this instance the loss of sight was toward my left, and was the same whether I looked with the right eye or the left. This blindness was not so complete as to amount to absolute blackness, but was a shaded darkness without definite outline. The complaint was of short duration, and in about a quarter of an hour might be said to be wholly gone, having receded with a gradual motion from the center of vision obliquely upwards toward the left.

Since this defect arose from over fatigue, a cause common to many other nervous affections, I saw no reason to apprehend any return of it, and it passed away without need of remedy, without any farther explanation, and without my drawing any useful inference from it.

It is now about fifteen months since a similar affection occurred again to myself, without my being able to assign any cause whatever, or to connect it with any previous or subsequent indisposition. The blindness was first observed, as before, in looking at the face of a person I met, whose *left* eye was to my sight obliterated. My blindness was in this instance the reverse of the former, being to *my right* (instead of the left) of the spot to which my eyes were directed; so that I have no reason to suppose it in any manner connected with the former affection.

The new punctum cæcum was situated alike in both eyes, and at an angle of about three degrees from the center; for, when any object was viewed at the distance of about five yards, the point not seen was about ten inches distant from the point actually looked at.

On this occasion the affection, after having lasted with little alteration for about twenty minutes, was removed suddenly and entirely by the excitement of agreeable news respecting the safe arrival of a friend from a very hazardous enterprise.

In reflecting upon this subject, a certain arrangement of the optic nerves has suggested itself to me, which appears to afford a very probable interpretation of a set of facts, which are not consistent with the generally received hypothesis of the decussation of the optic nerves.

Since the corresponding points of the two eyes sympathise in disease, their sympathy is evidently from structure, not from mere habit of feeling together, as might be inferred, if reference were had to the reception of ordinary impressions alone. Any two corresponding points must be supplied with a pair of filaments from the same nerve, and the seat of a disease in which similar parts of both eyes are affected, must be considered as situated at a distance from the eyes at some place in the course of the nerves where these filaments are still united, and probably in one or the other thalamus nervorum opticorum.

It is plain that the cord, which comes finally to either eye under the name of optic nerve, must be regarded as consisting of two portions, one half from the right thalamus, and the other from the left thalamus nervorum opticorum.

According to this supposition, decussation will take place only between the adjacent halves of the two nerves. That portion of nerve which proceeds from the right thalamus to the right side of the right eye, passes to its destination without interference; and in a similar manner the left thalamus will supply the left side of the left eye with one part of its fibres, while the remaining halves of both nerves in passing over to the eyes of the opposite sides must intersect each other, either with or without intermixture of their fibres.

Now, if we consider rightly the facts discovered by comparative anatomy in fishes, we shall find that the crossing of the entire nerves in them to the opposite eyes, is in perfect conformity to this view of the arrangement of the human optic nerves. The relative position of the eyes to each other in the sturgeon, is so exactly back to back, on opposite sides of the head, that they can hardly see the same object; they can have no points which generally receive the same impressions as in us; there are no corresponding points of vision requiring to be supplied with fibres from the same nerve. The eye which sees to the left has its retina solely upon its right side; and this is supplied with an optic nerve arising wholly from the right thalamus; while the left thalamus sends its fibres entirely to the left side of the right eye for the perception of objects situated on the right. In this animal, an injury to the left thalamus might be expected to occasion entire blindness of the right eye alone, and want of perception of objects placed on that side. In ourselves, a similar injury to the left thalamus would occasion blindness (as before) to all objects situated to our right, owing to insensibility of the left half of the retina of both eyes.

A disorder that has occurred within my own knowledge in the case of a friend, seems fully to confirm this reasoning, as far as a single instance can be depended upon. After he had suffered severe pain in his head for some days, about the left temple, and toward the back of the left eye, his vision became considerably impaired, attended with other symptoms indicating a slight compression on the brain.

It was not till after the lapse of three or four weeks that I saw him, and found that, in addition to other affections which need not here be enumerated, he laboured under a defect of sight similar to those which had happened to myself, but more extensive, and it has unfortunately been far more permanent. In this case the blindness was at that time, and still is, entire, with reference to all objects situated to the right of his center of view. Fortunately, the field of his vision is sufficient for writing perfectly. He sees what he writes, and the pen with which he writes, but not the hand that moves the pen. This affection is, as far as can be observed, the same in both eyes, and consists in an insensibility of the retina on the left side of each eye. It seems most probable, that some effusion took place at the time of the original pain on that side of the head, and has left a permanent compression on the left thalamus. This partial blindness has now lasted so long without sensible amendment, as to make it very doubtful when my friend may recover the complete perception of objects on that side of him.

In reviewing the several phenomena that I have described, we find partial blindness occurring at the same time in both eyes. This sympathy from disease is readily explained, on the supposition that the parts which sympathise receive their nerves from the same source, while the opposite halves of the eyes, which are not at the same time similarly affected, are supplied from an opposite source; and the inference is immediate, that in common vision also the sympathy of corresponding points, which receive similar impressions from the same object, is dependent on the arrangement of nerves thus detected by disease.

We find moreover in the sturgeon, (and it is the same in some other fishes) whose eyes can scarcely see the same object at once, and have no corresponding points which ordinarily sympathise, that the two eyes do not receive any nervous fibres from the same source; but one eye receives its nerve wholly from one side, and the other from the other side of the brain.

From the structure of these fish we learn distinctly, that the perception of objects toward one side is dependent on nerves derived from the opposite side of the brain; and in the last case of diseased vision above related, we find apparent injury to one side of the brain, followed by blindness toward the opposite side of the point to which both eyes are directed.

A series of evidence in such apparent harmony throughout, seems clearly to establish that distribution of nerves I have endeavoured to describe, which may be called the semi-decussation of the optic nerves.

On single vision with two eyes.

So long as our consideration of the functions of a pair of eyes is confined to the performance of healthy eyes in common vision, when we remark that only one impression is made upon the mind, though two images are formed at the same moment on corresponding parts of our two eyes, we may rest satisfied in ascribing the apparent unity of the impression to habitual sympathy of the parts, without endeavouring to trace farther the origin of that sympathy, or the reason why, in infancy, the eyes ever assume one certain direction of correspondence in preference to squinting.

But, when we regard sympathy as arising from structure, and dependent on connection of nervous fibres, we therein see a distinct origin of that habit, and have presented to us a manifest cause why infants first begin to give the corresponding direction to their eyes, and we clearly gain a step in the solution, if not a full explanation, of the long agitated question of single vision with two eyes.

It may perhaps to some persons appear surprising, that so many as three instances of a disorder which they presume to be rare, should have been witnessed by one individual; but I apprehend, on the contrary, this half-blindness to be far more common than is generally supposed; and I might with as much reason express surprise at its having so far escaped notice,* were I not aware how many facts commonly remain disregarded, merely for want of explanation. It is evident that I once, and for a long time, overlooked the inference that is to be drawn from this affection; and if the disorder had not happened to me a second time, I might never have reconsidered its cause.

Even since the preceding pages were written, I have met with two more cases of this disease. One of my friends has been habitually subject to it for sixteen or seventeen years, whenever his stomach is in any considerable degree deranged. In him the blindness has been invariably to his right of the center of vision, and, from want of due consideration, had been considered as temporary insensibility of the right eye; but he is now satisfied that this is not really the case, but that both eyes have been similarly affected with half-blindness. This symptom of his indigestion usually lasts about a quarter of an hour or twenty minutes, and then subsides, without leaving any permanent imperfection of sight.

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* RICHTER, in the third volume of his Elements of Surgery, has a chapter on half-blindness, and part of it relates to what he terms amaurosis dimidiata. From one instance there given, he seems to have seen some cases similar to those I have described; but he has not noticed the corresponding affection of the two eyes, or considered the sympathy between them.

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Vol. 3, Chap. 16, p. 478.

I have not seen the subject of the 5th case, but I am informed that he has had many returns of this affection, generally attended with head-ach, and always lasting about twenty minutes, with very little variation.

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